
NIL SISTERE CONTRA

THE

PHILOSOPHICAL TRANSACTIONS

(*From the Year* 1732, *to the Year* 1744)

ABRIDGED,

AND

Diſpoſed under GENERAL HEADS,

The *Latin* PAPERS being tranſlated into *Engliſh*.

By *JOHN MARTYN*, F. R. S.

Profeſſor of BOTANY in the Univerſity of *Cambridge*.

In TWO VOLUMES,

VIZ.

VOL. VIII. CONTAINING,

PART I. The MATHEMATICAL PAPERS.

PART II. The PHYSIOLOGICAL PAPERS.

VOL. IX. CONTAINING,

PART III. The ANATOMICAL and MEDICAL PAPERS.

PART IV. The HISTORICAL and MISCELLANEOUS PAPERS.

LONDON:

Printed for W. INNYS, C. HITCH, T. ASTLEY in *Pater-noſter-Row*, T. WOODWARD, C. DAVIS in *Holbourn*, and R. MANBY and H. S. COX on *Ludgate-Hill*. MDCCXLVII.

TO THE

PRESIDENT,

COUNCIL, and FELLOWS

OF THE

ROYAL SOCIETY

OF

LONDON,

For the improving of

NATURAL KNOWLEDGE,

This Abridgment of the PHILOSOPHICAL TRANSACTIONS is most humbly dedicated by

Chelsey, March 1,
1746-7.

JOHN MARTYN.

TO THE

READER.

As theſe Volumes are only a Continuation of thoſe, which were publiſhed twelve Years ago, by the late learned Mr *Eames* and myſelf, there does not ſeem to be any Occaſion for a Preface. The only Alteration now made, is the Addition of the Dates of the ſeveral Papers in the Margin, which I hope will be thought an Improvement. But as I apprehend, that this may ſometimes have been omitted, I ſhall endeavour to ſupply that Defect here, as I did in the former Volumes, by adding the following Table, which will ſhew for what Months each Tranſaction was publiſhed.

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THE
Philosophical Transactions
ABRIDGED.

PART I.
CONTAINING THE
Mathematical PAPERS.

CHAP. I.

Algebra, Arithmetick, Fluxions, Geometry.

I. I HAVE explained, in the Appendix to *Saunderson*'s Algebra, my Method of extracting any Root from the Binomial $a + \sqrt{-b}$; the reading of which has caused my learned Friend, *W. Jones*, Esq; *F. R. S.* to desire me to perform the same in the possible Binomial $a + \sqrt{+b}$. I shall obey his Commands in this particular, though I am very sensible, that this has been done already by Sir *I. Newton* and others.

Of the Reduction of Radicals to more simple Terms, by Mr A. de Moivre, *F. R. S.* N°. 451, *p.* 463. *Dec.* 1738.

To reduce the Binomial $\sqrt[n]{a + \sqrt{b}}$ *to more Simple Terms.* Prob. I.

Suppose that this Binomial, involved with it's general Radicality, can be reduced to the other Binomial, freed from it's general Radicality; now to find each Quantity x and y, try whether the Sum of the Binomials Solution.

Binomials $\sqrt[n]{a+\sqrt{b}}+\sqrt[n]{a-\sqrt{b}}$, which may be done by a Table of Logarithms, makes nearly a whole Number. If it be ſo, put $2x$ equal to this whole Number: then ſee whether $\sqrt[n]{aa-b}$ is a whole Number, if it is, put m equal to this new *Integer*, and there will be $y=xx-m$, wherefore the given Binomial will be reduced to the given Form. But before we proceed to the Demonſtration, it will not be improper to illuſtrate the thing by two or three Examples.

Example 1. Let the Binomial $\sqrt[2]{54+\sqrt{980}}$ be reduced to a more ſimple Term.

Put $a=54$, $b=980$; then $\sqrt{b}=\sqrt{980}=31,3049$ nearly, wherefore $a+\sqrt{b}$ will be $=85,3049$, and $a-\sqrt{b}=22,6951$.
The Square Root of the firſt Number is very near 9,236.
The Square Root of the latter is 4,763.
The Sum of the Roots is 13,999, to which the whole Number 14 is very near; therefore put $2x=14$, or $x=7$; now ſince $y=xx-m$, and $m=\sqrt{aa-b}=\sqrt{2916-980}=\sqrt{1936}=44$; therefore we ſhall have $y=49-44=5$, and ſo the Binomial reduced will be $7+\sqrt{5}$.

Example 2. Let $\sqrt[3]{45+\sqrt{1682}}$ be reduced to a more ſimple Term.

Put $a=45$, $b=1682$, therefore $\sqrt{b}=41,01219$ very nearly; therefore we ſhall have $a+\sqrt{b}=86,01219$, and $a-\sqrt{b}=3,8971$.
The Cube Root of the firſt Number is 4,4142; the Cube Root of the latter is 1,5857; the Sum of the Roots is 5,99991, which is very near the whole Number 6; therefore ſay $2x=6$, or $x=3$; but $y=xx-m$; but $m=\sqrt[3]{aa-b}=\sqrt[3]{343}=7$; and ſo $y=9-7=2$; therefore the Binomial reduced is $3+\sqrt{2}$.

Exapmle 3. Let $\sqrt[3]{170+\sqrt{18252}}$ be reduced to a more ſimple Term.

Put $a=170$, $b=18252$, then we ſhall have $\sqrt{b}=135,1$ very nearly; wherefore we ſhall have $a+\sqrt{b}=305,1$, and $a-\sqrt{b}=34,9$.
The Cube Root of the firſt Number is 6,73 very nearly.
The Cube Root of the latter is 3,26 very nearly.
The Sum of the Roots is 9,99, which is very near the whole Number 10; therefore ſay $2x=10$, or $x=5$, we have alſo $y=xx-m$; but $m=\sqrt[3]{aa-b}=22$; therefore $y=25-22=3$; therefore the Binomial reduced is $5+\sqrt{3}$.

Take any Binomial, as $\sqrt[3]{a+\sqrt{b}}$, which ſuppoſe reducible to the Binomial $x+\sqrt{y}$; therefore *Demonſtration.*

$$x^3+3xx\sqrt{y}+3xy+y\sqrt{y}=a+\sqrt{b};$$
ſay $x^3+3xy=a$,
and $3xx\sqrt{y}+y\sqrt{y}=\sqrt{b}$.

Whatſoever the Index of the Radicality ſhall be, from the Square of the firſt Part ſubtract the Square of the latter; now the Square of the former Part will be

$x^6+6x^4y+9xxyy=aa$;
The Square of the latter $9x^4y+6xxyy+y^3=b$;
The Remainder will be $x^6-3x^4y+3xxyy-y^3=aa-b$,

extract from both the Root, of which the Index is n, that is, in this Caſe, the Cube Root; therefore we ſhall have $xx-y=\sqrt[3]{aa-b}$, or to the *Factum* $\sqrt[3]{aa-b}=m$; we ſhall have $xx-y=m$; and therefore $y=xx-m$; now in the abovementioned Equation, namely, $x^3+3xy=a$, for y ſay $xx-m$, and you will obtain the Equation $4x^3-3mx=a$; here ſtop a little.

Now reſume the Equation $2x=\sqrt[3]{a+\sqrt{b}}+\sqrt[3]{a-\sqrt{b}}$, and ſuppoſe you would ſtrike out the Radicality $\sqrt[3]{\ }$;

In order to this, make $a+\sqrt{b}=z^3$,
and $a-\sqrt{b}=v^3$,

you will then have theſe two new Equations,

$$\begin{array}{l} z^3+v^3=2a \\ z\ +v\ =2x \\ \hline \end{array}$$
$$\frac{z^3+v^3}{z+v}\quad\frac{a}{x}$$

It follows therefore that $z+v$ x

But $\dfrac{z^3+v^3}{z+v}=zz-zv+vv$; therefore $zz-zv+vv=\dfrac{a}{x}$;
beſides $zz+2zv+vv=4xx$.

Take the Difference of theſe Equations, you will have $3zv=4xx-\dfrac{a}{x}$; but $z^3v^3=aa-b$; therefore $zv=\sqrt[3]{aa-b}$; but if you ſay $=m$, then it will be $3m=4xx-\dfrac{a}{x}$, or $4x^3-3mx=a$, which is the very Equation which came out before, and it will return to the ſame in every Caſe of Radicality whatſoever.

If therefore you would try whether the Expreſſion $\sqrt[n]{a+\sqrt{b}}$ can be reduced to a more ſimple Term; ſay $2x=\sqrt[3]{a+\sqrt{b}}+\sqrt[3]{a-\sqrt{b}}$; ſay alſo $\sqrt[n]{aa-b}=m$, and $y=xx-m$; and the Expreſſion reduced will be $x+\sqrt{y}$, if ſo be theſe can be done by integral, or at leaſt rational Quantities.

But in caſe theſe ſhould not be integral, or rational Quantities, yet the Rule which we have delivered, will be of Uſe in the Solution of Equations of any Kind, as will hereafter be ſeen.

In the mean time, this Doubt may perhaps ariſe, whether this Rule will obtain univerſally in any Powers whatſoever of a Binomial; for Inſtance, whether in any Binomial whatſoever, of which the Index is n, if from the Square of the Sum of thoſe Terms, which are in unequal Places, you ſubtract the Square of the Sum of thoſe which are in equal Places, the Remainder will be another Binomial, of which the Index alſo will be n.

To this I anſwer, that it has been obſerved by many before me, and therefore may be looked upon as confirmed by Experiments; but however, it may not be amiſs to produce a Demonſtration of it, which I do not remember to have ſeen any where.

Take the Binomial $\overline{x+y}|^n$ and expand it; take alſo another Binomial $\overline{x-y}|^n$, which expand in like Manner; ſay $\overline{x+y}|^n=s$, and $\overline{x-y}|^n=p$; now it will appear at firſt Sight, that, if the expanded Binomials are joined by Addition, their Sum will be equal to double the Sum of the unequal Terms of the firſt Binomial; but if the latter be ſubtracted from the former, that then the Remainder will be equal to double the Sum of the equal Terms of the firſt Binomial; hence it follows, that $\frac{s+p}{2}$ is the Sum of the unequal Terms; and $\frac{s-p}{2}$ the Sum of the equal Terms.

From the Square of the firſt Sum, that is, from the Square $\frac{ss+2ps+pp}{4}$ ſubtract the Square of the latter, namely, $\frac{ss-2ps+pp}{4}$ the Remainder will be $\frac{4ps}{4}=sp=\overline{x+y}|^n \times \overline{x-y}|^n=\overline{xx-yy}|^n$ of which the Root (the Index of which is n) is $=xx-yy$.

If

If you put $2x = \sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$, and take besides $\sqrt[n]{aa - b} = m$, and interpret n successively by 1, 2, 3, 4, 5, 6, 7, 8, *&c.* there will arise the following Equations. *Corollary.*

1. $x = a$.
2. $2xx - m = a$.
3. $4x^3 - 3mx = a$.
4. $8x^4 - 8mxx + mm = a$.
5. $16x^5 - 20mx^3 + 5mmx = a$.
6. $32x^6 - 48mx^4 + 18mmxx - m^3 = a$.
7. $64x^7 - 112mx^5 + 56mmx^3 - 7m^3x = a$, &c.

Now these Equations are of the same Form as the Equations to the Cosines, though they are naturally quite different.

Let r be the Radius of a Circle, l the Cosine of any given Arch, x the Cosine of another Arch, which may be to the first, as 1 to n.

1. there will be $x = l$.
2. $2xx - rr = rl$.
3. $4x^3 - 3rrx = rrl$.
4. $8x^4 - 8rrxx + r^4 = r^3 l$.
5. $16x^5 - 20rrx^3 + 5r^4x = r^4 l$.
6. $32x^6 - 48rrx^4 + 18r^4xx - r^6 = r^5 l$.
7. $64x^7 - 112rrx^5 + 56r^4x^3 - 7rx = r^6 l$, &c.

But the general Form of these is by putting for the Sake of Brevity $r = 1$

$$2^{n-1} \times x^n - 2^{n-3} \times \frac{n}{1} x^{n-2} + 2^{n-5} \times \frac{n}{1} . \frac{n-3}{2} x^{n-4} - 2^{n-7} \times \frac{n}{1} . \frac{n-4}{2} . \frac{n-5}{3} x^{n-6} + 2^{n-9} \times \frac{n}{1} . \frac{n-5}{2} . \frac{n-6}{3} . \frac{n-7}{4} \times x^{n-8} \text{ \&c.} = l.$$

The Difference of these Equations consists chiefly in this, that the first are derived from the Equation $2x = \sqrt[n]{a + \sqrt{b}} + \sqrt[n]{a - \sqrt{b}}$, but the latter from the Equation $2x = \sqrt[n]{a + \sqrt{-b}} + \sqrt[n]{a - \sqrt{-b}}$, and if this latter Equation be freed from it's general Radicality, we shall obtain Equations to the Cosines.

Let there be therefore the Equation $2x = \sqrt[3]{a + \sqrt{-b}} + \sqrt[3]{a - \sqrt{-b}}$, which must be freed from it's radical Sign $\sqrt[3]{}$.

Say

Say $\sqrt[3]{a+\sqrt{-b}}=z$, and $\sqrt[3]{a-\sqrt{-b}}=v$; say also $z+v=2x$. Hence you will have

1. $z^3=a+\sqrt{-b}$
2. $v^3=a-\sqrt{-b}$

hence it will be $z^3+v^3=2a$.

But $z+v=2x$, therefore it will be $\frac{z^3+v^3}{z+v}\ \frac{a}{x}$;

But $\frac{z^3+v^3}{z+v}=zz-zv+vv$; wherefore $zz-zv+vv$ will be $=\frac{a}{x}$.

But $zz+2zv+vv=4xx$; whence $3zv=4xx-\frac{a}{x}$; but now $z^3v^3=aa+b$.

Therefore it follows, that zv is $=\sqrt[3]{aa+b}$; which if you make $=m$, therefore $4xx-\frac{a}{x}$ will be $=3m$, or $4x^3-3mx=a$.

Hitherto we have had two Kinds of Equations; the first in which m was put $=\sqrt[3]{aa-b}$; the latter, in which it was $=\sqrt[3]{aa+b}$. Let us call the first Hyperbolical, the latter Circular.

Prob. II. *To extract the Cube Root from an impossible Binomial,* $a+\sqrt{-b}$.

Solution. Suppose that Root to be $x+\sqrt{-y}$, of which if you take the Cube, you will find it to be $x^3+3xx\sqrt{-y}-3xy-y\sqrt{-y}$.

Now put $x^3-3xy=a$.
and $3xx\sqrt{-y}-y\sqrt{-y}=\sqrt{-b}$.

Then by taking the Squares there will arise two other Equations;

$$x^6-6x^4y+9xxyy=aa$$
$$-9x^4y+6xxyy-y^3=-b.$$

Now take the Difference of the Squares, there will be $x^6+3x^4y+3xxyy+y^3=aa+b$; wherefore $xx+y$ is $=\sqrt[3]{aa+b}$: now say $\sqrt[3]{aa+b}=m$, whence $xx+y$ will be $=m$, or $y=m-xx$; now in the Equation $x^3-xy=a$, instead of the Quantity y, substitute it's Value $m-xx$, you will have $x^3-3mx+3x^3=a$, or $4x^3-3mx=a$,

$= a$, which is the very Equation, which had before been deduced from the Equation $2x = \sqrt[3]{a + \sqrt{-b}} + \sqrt[3]{a - \sqrt{-b}}$; but it does not follow, that in the Equation $4x^3 - 3mx = a$, the Value of the Quantity x may be known by the former Equation, as it consists of two Parts, each of which includes the imaginary Quantity $\sqrt{-b}$; but this will be best done by the help of a Table of Sines.

Therefore let the Cube Root be to be extracted from the Binomial $81 - \sqrt{-2700}$; say $a = 81$, $b = 2700$; now $aa + b = 6561 + 2700 = 9261$, of which the Cube Root $= 21$, which suppose $= m$, so that $3mx$ may be $= 63x$; the Equation therefore to be resolved, will be $4x^3 - 63x = 81$, and if this is compared with the Equation to the Cosines, namely, $4x^3 - 3rrx = rrl$; rr will be $= 21$; and therefore r will be $= \sqrt{21}$; and moreover, l will be $= \frac{a}{rr} = \frac{81}{21} = \frac{27}{7}$.

Therefore let there be an Arc of a Circle, of which the Radius is $= \sqrt{21}$, and the Cosine $= \frac{27}{7}$.

Let the whole Circumference be C, take the Arches $\frac{A}{3}$, $\frac{C - A}{3}$, $\frac{C + A}{3}$, which will easily be known by a Trigonometrical Calculation, especially if you make use of Logarithms, then the Cosines of the Arcs to the Radius $\sqrt{21}$, will be three Roots of the Quantity x; wherefore since y is $= m - xx$, they will therefore be so many Values of the Quantity y, and so the Cube Root will be triple of the Binomial $81 + \sqrt{-2700}$, but let us accommodate it to Numbers.

Say as $\sqrt{21}$ to $\frac{27}{7}$, so Radius of the Tables to Cosine of any Arc, to which Arc, suppose A to be equal; but that Arc will be found near $23^\circ, 42'$; hence the Arc C—A will be $327^\circ, 18'$, and C + A $392^\circ, 42'$, the third Parts of which will be $10^\circ, 54'$; $109^\circ, 6'$; $13^\circ, 54'$; but now as the first of them is less than a Quadrant, it's Cosine, that is, the Sine $79^\circ, 6'$, ought to be looked upon as positive; as both the others are greater than a Quadrant, that is, the Sines of the Arcs $19^\circ, 6'$; $40^\circ, 54'$, ought to be looked upon as negative; but by the Trigonometrical Calculation, it will appear that these Sines to Radius $\sqrt{21}$ will be 4,4999, — 1,4999, — 3,0000, or $\frac{9}{2}$ — $\frac{3}{2}$, — 3; whence there will be so many Values of the Quantity y, namely all those which

which $m - xx$ repreſents, that is, $21 - \frac{81}{4}$, $21 - \frac{9}{4}$, $21 - 9 = \frac{3}{4}$, $\frac{75}{4}$, 12, of which the Square Roots are $\frac{1}{2}\sqrt{3}$, $\frac{1}{2}\sqrt{3}$, $2\sqrt{3}$; wherefore 3 Values of the Quantity $\sqrt{-y}$ will be $\frac{1}{2}\sqrt{-3}$, $\frac{1}{2}\sqrt{-3}$, $2\sqrt{-3}$; whence the three Values of the Quantity $\sqrt[3]{81 + \sqrt{-2700}}$ will be $\frac{1}{2} + \frac{1}{2}\sqrt{-3}$, $-\frac{3}{2} + \frac{5}{2}\sqrt{-3}$, $-3 + \frac{1}{2}\sqrt{-3}$, and after the ſame Manner of proceeding will be found three Values of the Quantity $\sqrt[3]{81 - \sqrt{-2700}}$, namely theſe $\frac{9}{2} - \frac{1}{2}\sqrt{-3}$, $-\frac{3}{2} - \frac{5}{2}\sqrt{-3}$, $-3 - \frac{1}{2}\sqrt{-3}$.

There have been many, among whom was the famous *Wallis*, who have thought that thoſe cubic Equations, which are referred to a Circle, may be ſolved by the Extraction of a Cube Root from an imaginary Quantity, ſuch as, $81 + \sqrt{-2700}$, without having any Regard to the Table of Sines; but let them ſay what they will, it is all a mere Fiction, and begging of the Queſtion; for if you attempt it, you muſt neceſſarily run back to that Equation which you had taken to ſolve. But this cannot be done directly, without the help of a Table of Sines, eſpecially if the Roots are irrational; and this has been obſerved by many before me.

Prob. III. *To extract a Root, of which the Index is n, from an impoſſible Binomial* $a + \sqrt{-b}$.

Solution. Let the Root be $x + \sqrt{-y}$, then having made $\sqrt[n]{aa + b} = m$; alſo $\frac{n-1}{n} = p$, deſcribe, or ſuppoſe to be deſcribed, a Circle, of which the Radius is $\sqrt{m}$, and therein take any Arc A, of which let the Coſine be $\frac{a}{mp}$; let C be the whole Circumference. Take to the ſame Radius, Coſines of the Arcs $\frac{A}{n}$, $\frac{C-A}{n}$, $\frac{C+A}{n}$, $\frac{2C-A}{n}$, $\frac{3C-A}{n}$, $\frac{3C+A}{n}$, &c. till the Multitude of them is equal to the Number n; which being done, ſtop there; then all thoſe Coſines will be ſo many

many Values of the Quantity x; as for the Quantity y, that will always be $m - xx$.

I muſt not omit, though it has been mentioned already, that thoſe Coſines muſt be reckoned affirmative, of which the Arcs are leſs than a Quadrant, and thoſe negative, the Arcs of which are greater than a Quadrant.

Any Equation of the Kind of thoſe mentioned above being given, to know whether the Solution of it is to be referred to the Hyperbolical, or to the Circular Species. Prob. IV.

Let n be the higheſt Dimenſion of the Equation; divide the Coefficient of the ſecond Term by $2^{n-3} \times n$, and let the Quotient be $=m$; now ſee whether the Square aa be greater or leſs than the Power m; if the former Caſe ſhall happen, the Equation is to be referred to the Hyperbola; if the latter, to the Circle. Solution.

Let the Equation $16x^5 - 40x^3 + 20x = 7$ be given, where $n=5$, therefore $2^{n-3} \times n = 20$: Divide 40 by 20, the Quotient is $2 = m$, moreover $m^n = 32$, and the Square $aa = 49$; and as this is greater than the Power 32, the Conſequence is, that the Equation is to be referred to the Hyperbolical Species; but as in the Hyperbolical Caſe it was put $\sqrt[5]{aa - b} = m$, it follows, that $aa - b = m^5 = 32$, and ſo $b = aa - 32 = 49 - 32 = 17$: But now the Root of the Equation in this Caſe is $\frac{1}{2}\sqrt[5]{7 + \sqrt{17}} + \frac{1}{2}\sqrt[5]{7 - \sqrt{17}}$; but $\sqrt{17} = 4{,}123105$ nearly, therefore $7 + \sqrt{17} = 11{,}123105$, and $7 - \sqrt{17} = 2{,}876895$; moreover, the fifth Root of the former Number will be found $= 1{,}6221$, the fifth Root of the latter $= 1{,}2353$, the Sum of the Roots $= 2{,}8574$, the half Sum $1{,}4287$ is the Value of the Quantity x in the given Equation.

Now let the Equation $16x^5 - 40x^3 + 20x - 5$ be given; in which m is $= 2$, but $a = 5$; it is plain that the Square aa is leſs than the fifth Power of the Number 2; wherefore the Value of the unknown x cannot be obtained without the Quinquesection of an Angle; and this is performed by the Help of our general Theorem, mentioned before, by taking to the Radius $\sqrt{2}$, the Arc of which the Coſine is $\frac{a}{m^p} = \frac{a}{4} = \frac{5}{4}$, and that Arc will be found $27^\circ, 55'$, nearly, of which the fifth Part is $5^\circ\ 35'$; now if you take the Logarithm of that Coſine of the Arc to Radius 1, you will find it to be $9{,}9979347$; but ſince our Radius ought to be $\sqrt{2}$, add to the former Logarithm the Logarithm $\sqrt{2}$, that is $0{,}1515150$, the Sum will be $10{,}1484497$, out of

 which

which if you take 10, the Remainder, namely, 0,1484497, will be the Logarithm of the Number fought, which will therefore be 1,4075 very nearly, and in the fame Manner the other four Roots may be found.

Some few things remain to be obferved, which I fhall add in this Place.

If the Equation is of the Hyperbolical Kind, and befides n is an odd Number, there will be only one poffible Root, the reft will be impoffible; but if n is an even Number, there will be only one Value of the Square xx, the reft are impoffible.

If the Equation is of the circular Kind, all the Roots will be poffible.

In order to know how many affirmative Roots there will be, and how many Negative, in Equations to Cofines, let this Rule be obferved.

If n is an even Number, there will be as many affirmative Roots as negative.

If n is an odd Number, but fuch that $\frac{n+1}{2}$ is an even Number, the Number of Affirmatives will be $\frac{n-1}{2}$, the Number of Negatives $\frac{n+1}{2}$.

But if $\frac{n+1}{2}$ is an odd Number, the Number of Affirmatives will be $\frac{n+1}{2}$, of Negatives $\frac{n-1}{2}$.

A Demonftration of Newton's *Method of raifing any Polynomial to any Power, by Means of an affumed Binomial, by* J. Caftillioni, N°. 464. *Read* May 6, 1742.

II. Every Index is either Integer or Fraction; and thefe are either pofitive or negative. 1. If the Index is an Integer and pofitive, then to raife the Binomial to a Power, of which the Index is m, is nothing but writing the given Binomial, as often over as there are Units in m, and to draw all thefe Binomials in their Turns.

2. If the Index is a Fraction and pofitive, to raife the Binomial to the Power $\frac{r}{n}$ is to raife the given Binomial to the Power r, and, this Power being given, to feek the Quantity, which being given to the Power n, equals the Power of the given Binomial r.

3. But when the Index is negative, whether it is an Index or a Fraction, to raife the Binomial, we muft do as in N°. 1, or 2, and then Unity is to be divided by the Power found.

I take

I take a Binomial $p+q$, that it may shew me any Polynomial.

Between p^m and q^m there are as many Geometrical Means, in the Ratio $p \cdot q$, as there are Units in $m-1$.

Being to find these Terms, I note that p^m is to q^m in a compound Ratio of $p^m \cdot 1$, and $1 \cdot q^m$, also p to q has a Ratio compounded of $p \cdot 1$, and of $1 \cdot q$; but if there are two Serieses of Powers, in one of which, the Indices of the Power p decrease in the same arithmetical Proportion, of which the Difference is 1, by which the *Indices* of q increase in the second Series, there will be had a Series of continual Proportionals in the Ratio $p \cdot 1$, and $1 \cdot q$.

$$\text{So } p \cdot 1 :: p^{m} \cdot p^{m-1} \cdot p^{m-2} \cdot p^{m-3} \cdot p^{m-4} \cdots\cdots p^{m-m} = p^{0} = 1$$
$$1 \cdot q :: 1 \cdot q \cdot q^{2} \cdot q^{3} \cdot q^{4} \cdots\cdots q^{m} \cdot$$

Therefore the corresponding Terms being multiplied in their Turns,

$$p \cdot q :: p^{m} \cdot p^{m-1} q \cdot p^{m-2} q^{2} \cdot p^{m-3} \cdot p^{m-4} q^{4} \cdots q^{m} \cdot$$

Now I say, that $\overline{p+q}|^m$ is composed of the Terms found above, as is easily proved by their Generation.

Therefore all the Terms which are in $\overline{p+q}|^m$ disposed in Order, are in continual Proportion. And indeed any two Numbers following each other immediately, are as the first Term of a Binomial Root to the second. This appears by the Generation, for p any Number of Times multiplied, is to q as many Times drawn into p as $p \cdot q$.

Therefore the Number of all is $m+1$; but also in the decreasing arithmetical Series $m \cdot m-1 \cdot m-2 \ldots\ldots 0$ — the Terms are in Number $m+1$, or in the increasing $0 \cdot 1 \cdot 2 \cdot 3 \ldots\ldots m$; therefore the component Terms $\overline{p+q}|^m$ ought to have these Indices, or to be $p^{m}, p^{m-1} q \ldots\ldots q^{m}$.

But by the Laws of Multiplication, the Number of the Terms ought to be $2^m > m+1$, therefore in this Factum some repeated Terms must be found.

The common *Facta* (namely those of which the Multiplicator and Multiplicand consist of different Quantities) contain all the different Terms, because they are all formed of different Factors. In Powers therefore, it must be seen what Terms were different, unless the Factors were always the same, and how many of the different ones are made equal by the Restitution of Letters; for so we shall find how often every one ought to be repeated in the Power.

Now it appears, that if the Factors were always different, all the Terms also in the Product would be different.

But when the first in the Product is made only of the first of the Multiplicators, and the last of them is made of the last, these *Facta* will always

always be different, though the making Binomials are the same, because the first Term of the Binomial differs from the second.

But of the rest, some may be made equal, because they are composed of the first of the Efficients multiplied into the second, and joined in different Manners.

It must therefore be enquired, after how many different Manners the Quantities, of which the Number is given, may be joined.

In our Case, the Index of the Things is m, the different Things two, of which one is repeated s Times, the other t, so that $s + t = m$; therefore the Number of the Changes will be

$$\frac{m \,.\, m-1 \,.\, m-2 \,.\, m-3 \,.\,.\,.\,.\,.\,.\,.\, 1}{s \,.\, s-1 \,.\, s-2 \,.\,.\,.\,.\, 1 \,.\, t \,.\, t-1 \,.\, t-2 \,.\, t-3 \,.\,.\,.\, 1}$$

So let $t = 1$, $s = m - 1$, the Term will be $p^{m-1} q$, and it's Coefficient $\frac{m \, m-1 \,.\, m-2 \,.\, m-3 \,.\,.\,.\,.\, 1}{m-1 \,.\, m-2 \,.\, m-3 \,.\,.\,.\,.\,.\, 1} = m.$

Let $t = 3$, $s = m - 3$; it's Coefficient will be had $p^{m-3} q^3$, $= \frac{m \,.\, m-1 \,.\, m-2 \,.\, m-3 \,.\, m-4 \,.\,.\,.\,.\,.\,.\, 1}{1 \,.\, 2 \,.\, 3 \,.\, m-3 \,.\, m-4 \,.\, m-5 \,.\,.\,.\,.\,.\,.\,.\,.\, 1} = \frac{m \,.\, m-1 \,.\, m-2}{1 \,.\, 2 \,.\, 3 \,.}$, and so of the rest.

If any one shall doubt, whether the former Demonstration will prove that all the Terms are necessarily formed after so many Manners, as they may, and shall contend that it only shews that it may happen, I shall answer thus.

Certainly $\overline{p+q}|^m = \overline{p+q} \times \overline{p+q}|^{m-1}$; but amongst the Terms of this are $p^{m-n-1} q^n$ which will necessarily be multiplied into p and q, and $p^{m-n-1} q^n \times p = p^{m-n} q^n = p^{m-n} q^{n-1} \times q$, therefore $p^{m-n} q^n$ by all possible Ways will be made into $\overline{p+q}|^m$, if $p^{m-n-1} q^n$ and $p^{m-n} q^{n-1}$ are generated as many Ways as possible into $\overline{p+q}|^{m-1}$; which will necessarily be, if $p^{m-n-2} q^n$, and $p^{m-n} q^{n-2}$ are in the lower Power $\overline{p+q}|^{m-2}$, and so on always to the Square in which pp, pq, and qq are had, formed after as many Ways as possible, (4. II. *Euclid.*) therefore also in the former.

This reasoning requires, that I should shew the same also after a Manner something different.

We have now shewn that the Coefficient of the first is Unity.

The

The second Term $p^{m-1}q$ is formed of $p^{m-2}q \times p$, and $p^{m-1} \times q$, that is, of the first of the Roots multiplied into the second of $\overline{p+q}|^{m-1}$, and of the second of the Root into the first of $\overline{p+q}|^{m-1}$, therefore in $\overline{p+q}|^{m}$ there is $p^{m-1}q$ once more as often as the second is in $\overline{p+q}|^{m-1}$ which is there once more as often as the second in $\overline{p+q}|^{m-2}$ which again is there once more as often as the second is in $\overline{p+q}|^{m-2}$ and so always till you come to $\overline{p+q}|^{m-m}$, where the second is once; therefore you must seek the Sum of as many Units as there are in m, which is m.

Also the third $p^{m-2}qq$ is formed of $p^{m-3}qq \times p$, the third of $\overline{p+q}|^{m-1}$ into the first of the Root, and of $p^{m-2}q \times q$ the second of $\overline{p+q}|^{m-1}$ into the second of the Root; therefore $\overline{p+q}|^{m}$ will contain $p^{m-2}qq$ as often as the second is contained in $\overline{p+q}|^{m-1}$, that is $m-1$ times more, as often as the third is there, that is, as often as the second is in $\overline{p+q}|^{m-2}$ $(m-2)$ more than the third is there, which again is as often as the second is in $\overline{p+q}|^{m-3}$ $(m-3)$ more than the third is there, and so on till we come to $\overline{p+q}|^{2}$ where the third is once, or to $p+q$, where there is no third; for we must always seek the sum of the arithmetical Progression $m-1 \,.\, m-2 \,.\, m-3 \,.\,.\,.\,.\,.\,.\, 1$, or $m-1 \,.\, m-2 \,.\,.\,.\,.\,.\,.\, 0$, in the former the Number of the Terms is $m-1$, in the latter m, as is manifest; wherefore this Sum $= \overline{m-1+1}$ $\times \frac{m-1}{2} = m \times \frac{m-1}{2} = \overline{m-1+0} \times \frac{m}{2}$.

By the same Means, the Coefficients of the other Terms will be proved to make the Series, in which the second Differences are in arithmetical Progression, *&c.*

Whence always, where m is an Integer and positive, the Formula will be $p^{m} + mp^{m-1}q + \frac{m \,.\, m-1}{2} p^{m-2}qq + \frac{m \,.\, m-1 \,.\, m-2}{2 \,.\, 3}$ $p^{m-3}q^{3} + \frac{m \,.\, m-1 \,.\, m-2 \,.\, m-3}{2 \,.\, 3 \,.\, 4} p^{m-4}q^{4} +$ $\frac{m \,.\, m-1 \,.\, m-2 \,.\, m-3 \,.\, m-4}{2 \,.\, 3 \,.\, 4 \,.\, 5} p^{m-5}q^{5}$, *&c.*

If we make $p+q=p\times\overline{1+\frac{q}{p}}$ there will arise the very *Formula* of Sir *Isaac Newton*; for $\overline{p+q}\rceil^{m}=p^{m}\times\overline{1+\frac{q}{p}}\rceil^{m}=p^{m}\times$ $\overline{1+\frac{m}{1}\times\frac{p}{q}+\frac{m.\overline{m-1}.}{1.2}\times\frac{p^{2}}{q^{2}}+\frac{m.\overline{m-1}.\overline{m-2}}{1.2.3.}\times\frac{p^{3}}{q^{3}}}$,&c. = (if A, B, C, D, *&c.* are supposed to equal the first, second, third, fourth, *&c.* each with their Coefficients) $p^{m}\times 1+m\,A\frac{q}{p}+\frac{m-1}{2}$ $B\frac{q}{p}+\frac{m-2}{3}\,C\frac{q}{p}+\frac{m-3}{4}\,D\frac{q}{p}+\frac{m-4}{5}\,E\frac{q}{p}+\frac{m-5}{6}\,F\frac{q}{p}$&c.

Let us now seek the *Formula*, or raising the same Binomial to the Power $\frac{r}{n}$, where r and n are whole Numbers, and both either positive or negative.

Now $p.q::p^{\frac{r}{n}}.x=\frac{p^{\frac{r}{n}}q}{p}=p^{\frac{r}{n}-1}q$, wherefore the Terms will be $p^{\frac{r}{n}}.\;p^{\frac{r}{n}-1}q.\;p^{\frac{r}{n}-2}qq.\;p^{\frac{r}{n}-3}q^{3}$, *&c.*

The Coefficients to be found are A, B, C, D, E, so that the whole $\overline{p+q}\rceil^{\frac{r}{n}}$ Root $=Ap^{\frac{r}{n}}+Bp^{\frac{r}{n}-1}q+Cp^{\frac{r}{n}-2}qq+Dp^{\frac{r}{n}-3}q^{3}+Ep^{\frac{r}{n}-4}q^{4}$, *&c.* therefore $\overline{p+q}\rceil^{r}$ $(p^{r}+rp^{r-1}q+\frac{r.\overline{r-1}}{2}p^{r-2}qq+\frac{r.\overline{r-1}.\overline{r-2}}{2.3}p^{r-3}q^{3}$, *&c.*$)=\overline{Ap^{\frac{r}{n}}+Bp^{\frac{r}{n}-1}q+Cp^{\frac{r}{n}-2}qq,\ \&c.}\rceil^{r}=A^{n}p^{r}+nA^{n-1}Bp^{r-1}q+n$ $A^{n-1}Cp^{r-2}qq+nA^{n-1}Dp^{r-3}q^{2}+nA^{n-1}Ep^{r-4}q^{4}$,&c. $+\frac{n.\overline{n-1}}{2}A^{n-2}B^{2}p^{r-2}qq+n.\overline{n-1}A^{n-2}BCp^{r-3}q^{3}+$ $n.\overline{n-1}A^{n-2}BDp^{r-4}q^{4}$, *&c.* $+\frac{n.\overline{n-1}.\overline{n-2}}{2.3.}A^{n-3}B^{3}$ $p^{r-3}q^{3}+\frac{n.\overline{n-1}.\overline{n-2}.\overline{n-3}}{2.3.4.}A^{n-4}B^{4}p^{r-4}q^{4}$. And

therefore

therefore the Terms being compared $1 = A^{n} = A^{n-1} = A^{n-2}$, &c. $nB = r$, and $B = \frac{r}{n}$, $nC + \frac{n.\overline{n-2}}{2} \times \frac{rr}{nn} = \frac{r.\overline{r-1}}{2}$, and $C = \frac{r.\overline{r-n}}{2.nn}$, $nD + n.\overline{n-1} \times \frac{r}{n} \times \frac{r.\overline{r-n}}{2nn} + \frac{n.\overline{n-1}.\overline{n-2}}{2.3} \times \frac{r^3}{n^3} = \frac{r.\overline{r-1}.\overline{r-2}}{2.3}$, and $D = \frac{r.\overline{r-n}.\overline{r-2n}}{2.3.n^3}$, &c.

If therefore we make $\frac{r}{n} = m$, and the first Term A, &c. the first *Formula* will revive, and $\overline{p+q}|^{\frac{r}{1}} = \overline{p+q}|^{m} = p^{m} \times \overline{1 + mA\frac{q}{p} + \frac{m-1}{2}B\frac{p}{q} + \frac{m-2}{3}C\frac{q}{p}}$, &c.

Let the Binomial $p+q$ be to be raised to the negative Power, either perfect or imperfect, $-s$.

Now $\overline{p+q}|^{-1} = \frac{1}{\overline{p+q}|^{1}} = \overline{p^{s} + sp^{s-1}q + \frac{s.\overline{s-1}}{2}p^{1-2}qq}$, &c. = (by Division) $\frac{1}{p} - \frac{sp^{s-1}q}{p^{2s}} - \frac{s.\overline{s-1}}{2} \times \frac{p^{s-2}qq}{p^{2s}} - \frac{s.\overline{s-1}.\overline{s-2}}{2.3} \times \frac{p^{s-3}q^{3}}{p^{2s}} - \frac{s.\overline{s-1}.\overline{s-2}.\overline{s-3}}{2.3.4} \times \frac{p^{s-4}q^{4}}{p^{2s}}$ $= p^{-s} - sp^{-s-1}q - \frac{s.\overline{s-1}}{2}p^{-s-2}qq$.

From this *Formula*, by insisting on the former Methods, is easily drawn the usual and most general $p^{m} \times \overline{1 + mA\frac{q}{p} + \frac{m-1}{2}B\frac{q}{p}}$, &c.

I do not think it an unpleasant thing, that in this *Formula*, if $m = -2$, the coefficient Numbers will be natural, if $m = 3$, Triangular; and Pyramidal, if $m = 4$, &c.

But it is plain, that this *Formula* always gives an infinite Series; for (if m expounds a positive Number) the last Term ought to be q^{-m}; but

but $p . q :: p^{-m} \; p^{-m-1} :: p^{-m-1} \; q . p^{-m-2} \; qq$, &c. therefore the Ratio of $p^{-m} . q^{-m}$ ought to be compounded of some Ratios $p . q$. which cannot be done, because $p^{-m} . q^{-m} :: \frac{1}{p^m} . \frac{1}{q^m} :: q^m . p^m$ in a Ratio compounded of the Reciprocals of $p . q$.

The Indices of p make an arithmetical Progression, of which the Terms $-m$, $-m-1$, $-m-2$, *&c.* are negative indeed, but increase or decrease from 3; but the last Term ought to be $q^{-m} = p \; q^{-m}$, therefore it will never come to it.

Description and Use of an Arithmetical Machine, invented by Christian-Ludovicus Gersten, *F. R. S. Prof. Math. at* Giessen. No. 438. p. 79. *July, &c.* 1735. *Fig.* 1, 2, 3, 4, 5.

III. Sir *Samuel Morland* was, for ought I know, the first who undertook to perform Arithmetical Operations by Wheel-work. To this end he invented two different Machines, one for *Addition* and *Substraction*, the other for *Multiplication*, which he published in *London*, in the Year 1673, in *small Twelves*. He gives no more than the outward Figure of the Machines, and shews the Method of working them. But as by this every one, who has any Skill in Mechanicks, will be able to guess, how the inward Parts ought to be contrived; so it cannot be denied, that these are two different Machines, independent of one another; that the last, which is for *Multiplication*, is nothing else but an Application of the *Nepairian* Bones on flat moveable Disks; consequently that his Invention alone is not fit to perform justly all Arithmetical Operations.

After him the celebrated Baron *de Leibnitz*, the Marquis *Poleni*, and Mr *Leupold* took this Undertaking in hand, and attempted to perform it after different Methods.

The first published his Scheme in the Year 1709, in the *Miscellanea Berolinensia*, but then he gave only the outward Figure of the Machine. S. *Poleni* communicated his, but explaining at the same time it's inward Construction, in his *Miscellanea* of the same Year, 1709. Mr *Leupold's* Machine, together with those of Mr *de Leibnitz* and S. *Poleni*, were inserted in his *Theatrum Arithmetico-Geometricum*, published at *Leipzig* in 1727, after the Author's Death, yet imperfect, as it is owned in the Book itself.

Besides these, I learned from several *French* Journals, that Monsieur *Pascal* invented one, which however I never had the sight of. *

I took the Hint of mine from that of Mr *de Leibnitz*, which put me upon thinking how the inward Structure might be contrived: But as it was

* The Description of this Machine is since printed by Monf. *Gallen*, in his Collection of Machines and Inventions approved by the *Academy of Sciences*, at *Paris*, (published in *French* at *Paris*. 1735, in Quarto, in six Tomes) in Tom. IV. p. 137; and likewise another by Monf. *Lespine*, Tom. IV. p. 131; and three more by Monf. *Hellerin de Boistissandeau*, Tom. V. p. 103, 117, and 121.

was not poſſible for me to hit upon the original Ideas of that Great Man, an exact Enquiry into the Nature of Arithmetical Operations furniſhed me at laſt with others, which I expreſſed in a rough Model of Wood, and ſhewed to ſome Patrons and Friends, who encouraged me to have another made of Braſs: But the want of an Artificer, able enough to execute my Ideas, made me delay it till the Year 1725; when having ſpare Time, and finding an Inclination to divert myſelf with Mechanical Operations, I ſet about it, and finiſhed the whole Work fitted to a Reckoning not exceeding ſeven Places. And in *Dec.* of the ſame Year, I had the Honour to lay this Machine before the preſent Landgrave of *Heſſen Darmſtadt*, and the Hereditary Prince his Son, to whom I demonſtrated the Mechaniſm of the whole Invention.

I was checked from publiſhing it at that time by the Uncertainty I was under, whether poſſibly Mr *Leibnitz*'s Machine had not been brought to it's Perfection; in which caſe there is no doubt but the Operation of his Machine, if it would really perform what is promiſed in the Deſcription, would have been eaſier than mine, and conſequently preferable to it, provided it's Structure did not prove too intricate, nor that the working of it took up too much time.

But at preſent, being certain that none of Mr *Leibnitz*'s Invention has yet appeared in ſuch a State of Perfection, as to have anſwer'd the Effect propoſed, and that theſe of mine differ from all thoſe mentioned above, fancying at the ſame time, that Perſons who underſtand Mechanicks, will find it plain, practicable, and exact, in regard to it's various Effects, I make no Scruple to preſent this Invention to the *Royal Society*.

The Particulars of it are as follows:

There are as many Sets of Wheels and moveable Rulers as there are Places in the Numbers to be calculated. *Fig.* 1. ſhews three of them. Fig. 1.
by which one may eaſily conceive the reſt. A A ſhews the firſt Syſtem or the Figures of Unites, according to it's inward Structure. BB and CC ſhews the ſecond and third Syſtem, *viz.* of Tens and Hundreds, accord- to their outward Form. We ſhall firſt conſider A A; where $\alpha\,\alpha\,\alpha$ is a flat Bottom of a Braſs Plate, which may be ſkrewed on either upon a particular Iron Frame, or only upon a ſtrong Piece of Walnut-Tree, doubled with the Grain croſs'd. In this Syſtem are two moveable Rules *gggg*, and *kkk*, the firſt of which I call the Operator, the ſecond the Determinator. There are beſides two Wheel-Works, the upper one is for *Addition* and *Subſtraction*, the lower one ſerves for *Multiplication* and *Diviſion*. The upper one is provided firſt with *a*, an oblique Ratchet-Wheel of 10 Teeth, of what Diameter you pleaſe, on which, however, depends the Length and Breadth of the Syſtem itſelf. This Wheel has a Stop *r*, with a depreſſing Spring *t*; Under the Wheel *a* is a ſmaller Wheel *b* of the ſame Shape: Both *a* and *b* are rivetted together, and fixed on a common Axis. Under the Wheel *b* lies a third *f*, which is a common Tooth-Wheel of 20 or more Teeth, according as one pleaſes: It is larger than *b*, and ſmaller than *a*, turns about the ſame Axis with

 the

the other two above it, and upon it is fixed a Stop *c*, with the Spring *d*, which catches the oblique Teeth of the Wheel *b*. Immediately under this Wheel lies the upper Part of the Operator, which may be beft made of Iron or Steel. The Wheels may all be of Brafs, except the upper one. The Operator is of the fame Thicknefs all over, and in it's upper Part are fixed as many round Steel Pins as there are Teeth in the Wheel *f*, which are to catch the Teeth of this Wheel, and move it backwards and forwards. The Height of thofe Pins ought exactly to anfwer the Thicknefs of the Wheel *f*. The Axis of the Wheels *a* and *b* is kept perpendicularly by the Bridge *ee*, which is fkrewed to the Bottom, as appears by the Figure. The Operator *gggg* moves on the Side, above, and in the Middle in two Brafs Grooves *i i i* and *q q*; about D it jets out, on which Projection a Piece of Iron *h* muft be well faftened, having a ftrong Pin, on which the Handle *z* fits as you fee in the Syftem B B. The Side D itfelf flides in another Groove *s s*, and in it's inner Corner joins to it the Determinator *k k k*, of the fame Thicknefs with the Operator, the Shape of which is fufficiently expreffed in the Figure. This flides alfo up and down, on the one Side in the Groove *s s*, and *v* on the other Side, where it is fmalleft, in a fmall Piece of Brafs ♃, and where it is broadeft, above in the Operator itfelf, which is either hollowed out into another Groove, or filed off obliquely. The fliding part of the Determinator ought afterwards alfo to be fitted to it. It's chief Part is the Lock *u*, ftanding perpendicular on it's broad Part. I have drawn it feparately in *Fig.* 4. B B, in which the fliding Stop *c*, is preffed down by it's Spring *d*, but raifed by the Tricker *a a*. That Tricker has a pin *b*, on which is fkrewed on the fmall Handle *l l* (*Fig.* 1. in the Syftems B B and C C.) In the Brafs Bottom A A (or *α α Fig.* 1.) you muft file out 10 Ratchet-Teeth or Kerfs, purpofely for the Stop of this Lock, or, which is better, you may infert into the Brafs Bottom a fmall Piece of Iron filed out according to this Figure. The Partition and Length of thefe Rachet-Teeth in the Bottom muft fit exactly with the Circumference of the Wheel *f*, (*Fig.* 1. *Syftem* A A,) with this Direction, that if the Lock is kept by the uppermoft Tooth in the Bottom, the Operator cannot be moved at all; but when by preffing down the Tricker *a a*, (*Fig.* 4.) the Determinator is fhoved down, and is ftopp'd by the fecond or third Tooth in the Bottom, the Operator being alfo drawn down as far as the Determinator permits, makes the Stop *c*, (*Fig.* 1. *Syft.* A A) flide over 1 or 2 Teeth of the fecond Wheel *b*; confequently the fame Stop *c*, muft flide over 9 Teeth, when the Lock of the Determinator will ftand before the 10th Tooth in the Bottom, and the Operator is pulled down fo far. If you have a mind to apply thefe Rachet-Teeth on the Outfide of the Plate ⊙ ⊙, that covers the Whole, you may fit the Lock to it accordingly: But in this Cafe the Covering-Plate muft be well faftened.

For *Multiplication* and *Divifion*, there is properly in each Syftem but one Wheel, likewife divided into 10 Rachet-Teeth, on which is rivetted the round Plate *l*, on which are engraved the Numbers or Figures: Thefe Wheels

Fig. 4.

Wheels have no occaſion for any Bridge, but may turn about a ſtrong Pin of Steel, ſolder'd to the Bottom. The Rachet Wheel *m m* reſts on one Side upon the Determinator, and upon a Piece of Braſs of the ſame Thickneſs, to which are faſtened the Stop *n*, and the Spring *p*. Upon the Operator is another Stop *o*, with it's Spring; which Stop has a ſmall Arm at *o*, which is checked by a ſmall Studd, to hinder the Spring's preſſing the Stop lower down than it ought: By which Contrivance it is ſo order'd, that after the Operator is ſlid down ſo far as it can go, in being ſlid up again, the Stop *o* will turn but one Tooth of the Wheel *m m*. The round Plate *l* has in it's Middle a ſmall hollow Axis, on which are turned firſt two Shoulders, and then a Skrew: This Skrew in the Syſtem A A is an ordinary one, winding from the left to the right.

But as each Syſtem ought to have Communication with the preceding one, though not with that which follows; to this end a projecting Tooth of communication made of Steel ℘ is rivetted to the upper Plane of the uppermoſt Wheel *a*. This Tooth muſt be placed exactly on the Point of a Tooth of the Wheel, and by it's Revolution catches and turns every time but one Tooth of the uppermoſt Wheel of the preceding Syſtem, ſliding over the following one (if there be any) without touching it. For this reaſon the Planes of the Braſs Bottoms in all the Syſtems ought to incline a little. This will beſt appear from the Vertical Section, *Fig.* 2. (cut in *Fig.* 1. in the Direction from ♄ to *f*) in which *α* is the Braſs-Bottom, H H the Wood-Bottom, *g* the Operator, *i* the Groove, *f* the third common Tooth-Wheel, *b* the ſecond Wheel, *a* the firſt or uppermoſt Ratchet-Wheel, *e* the Bridge, ⊙ the Covering-Plate, and ℘ the Tooth of Communicaton. I have repreſented all theſe Pieces of one Thickneſs; but every Artiſt will eaſily know where to add or take off. Fig. 2.

Fig. 5. ſhews the Plan and true Diſpoſition of the Teeth in the ſeveral uppermoſt Wheels; that is to ſay, The Parallel Lines A B and C D ought always to cut the Braſs Bottoms (which are like one another in Length and Breadth) length-wiſe into two equal Parts: Then the perpendicular Interſection E F will determine the Centers *a* and *b*, of the two Wheels H and G. The ſtop *r* ought every time to hold it's Wheel in ſuch a manner, that the Points of two Teeth coincide with the Line A B or C D. The Obliquity of the Teeth is the ſame in both, with this difference however, that in G, which is a Wheel of the Syſtem A A, (*Fig.* 1.) they are cut in from the left to the right, but in H (a Wheel of the Syſtem B B) from right to the left. I need not take notice, that for making the Work more durable, the Teeth are not to be cut out into quite ſharp Points, but blunted a little, as in the Wheel H. The Nicety of the whole Machine chiefly conſiſts in placing the Center *a* and *b*, or (which amounts to the ſame thing) after having choſen the Breadth of the Braſs-Bottoms, in determining the Diameter of the uppermoſt Wheel: For if that ſhould prove ſo large, as that the two Wheels H and G ſhould very near touch one another, the Tooth of Communication will be ſhort, it's Operation will be of a ſmall Force, and the Wheels themſelves will Fig. 5.

 require

require a very great Exactneſs, leſt by turning about the Wheel H, and the Tooth of Communication ſtanding in the Poſition as it is repreſented in *Fig.* 5, a Tooth of the Wheel H may touch it, and ſtop the Motion. Whereas, on the other hand, ſuppoſing the Centers at the ſame Diſtance, and the Diameters of both Wheels leſs, the Tooth of Communication will be longer: than ſuch an Exactneſs is not requir'd in the Wheel, yet more Force is neceſſary for making the Tooth of Communication lay hold the better. Furthermore, it will be well for you to make the undermoſt common Tooth-Wheel as large as you can.

From the Conſtruction of this firſt Syſtem, with which the 3d, 5th, 7th, *&c.* entirely agree, one may eaſily imagine the 2d, 4th, 6th, 8th, *&c.* for every thing there alſo is the ſame, except only, that it is inverted; ſo that what in the firſt ſtands on the right-hand, is on the Left in the ſecond.

The Plate for *Multiplication* has on it's hollow Axis, as it is ſaid before, two Shoulders, the lowermoſt of which is very ſmall, the Sum of it's Height, the Thickneſs of the Plate of the Wheel *m m*, and of the Operator muſt amount to as much as anſwers to the Height of the Bridge *e e*. On both Ends of the Braſs-Bottoms, the two Pieces of Braſs ☾ ☾, of the ſame Height, are rivetted on. This being done, at laſt the Covering-Plates ⊙ ⊙ is prepared and ſkrewed on the Pieces of Braſs ☾ ☾. If the Machine be made pretty large, the Covering-Plate muſt be ſkrewed faſt, not only to the Bridge *e e*, but alſo not far from the Wheel of Multiplication. It muſt be provided not only with round Holes, through which are to go the Axis of each uppermoſt Wheel *a*, and the hollow Axis of the Plate *l*; but it muſt alſo have a long Slit, in which the Operator and Determinator may be moved up and down, and laſt of all a ſmall Window over the Plate of *Multiplication*, through which the Figure or Number engraved on the Plate may appear diſtinctly. To the projecting Skrew *l*, of the Plate *l*, is fitted an Handle *ff*, joined to an Index in the Shape of a Scythe. The Skrew in the Syſtem A A is a common Skrew, conſequently the Roundneſs of the Scythe muſt turn from the left to the right; but in the Syſtem B B, where it ought to be inverted, like all the other Parts, the Scythe muſt turn from the right to the left, as in the Figure. The Uſe of this is to ſhew which Way the Wheels are to be turned; and the Skrews are to prevent the Machine's being hurt by unſkilful Hands.

On the Side of the Determinator, *viz.* on that Piece which cannot be preſſed down, is alſo ſkrewed a ſmall Index, which may be directed to ſuch Numbers or Figures as is required. Theſe Figures are to be engraven in the Covering-Plate, according to the Figure, and their Diſtance depends on the Ratchet-Teeth *e e* (*Fig.* 4.) in the Braſs-Bottom.

On the Axis of each uppermoſt Wheel *a* (which Axis muſt be made ſquare as far as it projects over the Covering-Plate) is fixed a thin round Silver-Plate *x x* (in the Syſtems B B and C C) or *a d* in *Fig.* 3. yet ſo that it may not rub againſt the Covering-Plate. It has a hollow Axis *b c*

(*Fig.*

(Fig. 3.*)* on which is a right or left Skrew, according to the Syſtem it belongs to, and a ſmall Shoulder *c.* To the Skrew is ſkrewed the Handle *fs* (Syſtem BB and CC, *Fig.* 1.) which is vertically flat on the Extremity, in order to turn by it the Plate and the Wheels. The Plate (as appears by the Figure) is divided by 3 concentrick Circles into two Rings, in the outmoſt of which are engraven the Numbers for *Addition*, in the inmoſt thoſe for *Subſtraction.* I will hereafter call this Plate only the *Silver-Plate*, the firſt Ring the *Addition-Ring*, the ſecond the *Subſtraction-Ring*: Moreover two Indexes *w* and *y* are ſkrewed to the Covering-Plate; *w* ſhews the Numbers of the outmoſt or *Addition Ring*, and *y* thoſe of the *Subſtraction-Ring.* They have Hinges, that they may be lifted up, and the *Silver-Plate* taken out or put in again: Their Curvature ſerves for a Direction, which way the Plates ought to be turned. A ſkilful Artificer will be able to give them a neater and handſomer Shape, than here in the Draught, where I would not cover the Numbers.

All this being done, there remains now the Figures or Numbers to be engraven, in the manner following: Place each uppermoſt Wheel *a* (Syſtem A A) ſo that the Tooth of Communication be ready to catch (as in G, *Fig.* 5) which may be eaſily felt. Obſerve in the Silver-Plate, where the Index *w* points, and there engrave the Number or Figure 9, lower down in the *Subſtraction-Ring*, where the Index *y* points, engrave the Cypher *o*. After this divide both Rings into 10 equal Parts, one of which is already deſigned for 9 in the *Addition*, and another for *o* in the *Subſtraction-Ring*; then obſerve which way the Wheel turns, if from the right to the left, as in Syſtem B B, then you muſt from the engraven Number 9 in the *Addition-Ring*, towards the right engrave *o* next, then 1, 2, 3, 4, *&c.* and in the *Subſtraction-Ring* towards the right alſo, from the already engraved *o*, firſt engrave 9, then 8, 7, 6, *&c. ordine inverſo.* But if the Wheel turns from the left to the right, as in the Syſtems A A and C C, you engrave the Numbers or Figures in the ſame Order, but from the right to the left. (See in *Fig.* 1. the *Syſtems* BB and CC.)

In the *Multiplication-Wheels mm* you muſt conduct the Index *ff* exactly to the Window, as it is drawn in the Syſtem BB; mark the Place on the round *Multiplication-Plate* under the Window, and engrave upon it the Cypher or *o*; Then make, by two concentrick Circles, a Ring upon this Plate, and divide this Ring into ten equal Parts, and after the *o* (already engraven) engrave on the Numbers 1, 2, 3, 4, 5, 6, 7, 8, 9, in the ſame Order as it was done in the *Addition-Ring* of the Silver-Plate of the ſame Syſtem. Laſt of all, if you think fit, you may ſkrew on thin Ivory Plates, to note upon them the Numbers which are to be calculated, particularly a long ſmall one on that Side of the Slit of the Determinator, where there are are no Numbers, and alſo two ſhorter broader ones, one under the Window of *Multiplication*, the other above the Silver-Plate. All this together compoſes a Machine, by the help of which you may perform all the four Arithmetical Rules or Operations. The Way of working it, is as follows:

I. *As to addition:* For inſtance, if you are to add 32 and 59: becauſe the hindmoſt Syſtem A A in the Figure, which ought to repreſent the Place of Unites, is not cover'd, let us take the Syſtem BB for the Place of the Unites, and the Syſtem CC for the Place of the Tens; turn the Silver-Plates *xx* in theſe two Syſtems, that the Indexes *ww* point to the two Numbers 5 and 9; then make the Determinators *ll*, *ll*, point alſo to 3 and 2: next take one of the two Operators, *ex. gr.* in BB, and pull it down as far as you can, and moveit upwards again. This done, the Number 1 of the Silver-Plate in BB will come by this means under the Index *w*, and the Number 6 of the Silver-Plate in the Syſtem CC under it's Index at the ſame time, which is 61, the Sum, 59 and 2. After this move the Operator of the Syſtem CC alſo up and down, when inſtead of 6, 9 will come under the Index; conſequently you have 91 under the Indexes *ww*, which is the Sum requir'd of 59 and 32 added together. The Reaſon of it is plain; for by pulling down the Operator of the Syſtem BB ſo far, the Stop *c* of the lowermoſt or common Tooth-Wheel *f* (*vid.* Syſt. A A) will ſlide over two Teeth of the Rachet-middlemoſt Wheel *b*; and by moving the Operator up again, the ſame Stop *c* will turn the two Ratchet-Wheels *a* and *b* together, and cauſe the Stop *r* of the great or uppermoſt Wheel *a* to ſlide alſo over two Teeth; at the ſame time the Tooth of Communication *ℓ* will move forward one Tooth of the uppermoſt Ratchet-Wheel in the Syſtem CC; conſequently on the Silver-Plate in BB, inſtead of 9 the Number 1, and in Syſtem CC, inſtead of 5 the Number 6 muſt appear under their Indexes *ww*; and ſo for the ſame reaſon, having pulled up and down the Operator of the Syſtem CC, the Number 6 pointed to by the Index muſt be at laſt changed into 9.

II. *Subſtraction.* Suppoſe 40 the Sum, from which you are to ſubſtract 24: Here you muſt put your Sum 40 in the *Subſtraction-Rings*; that is to ſay, turn the Cypher *o* in the Syſtem BB, and the Number 4 in the Syſtem CC, under the Indexes *yy*, as the Figure ſhews: Set the Determinators at 24, as in *Addition*; move alſo the Operators only once up and down, the Remainder 16 will appear under the Indexes *yy*. As for the Reaſon of this Operation, when you conſider, that the Numbers in the *Subſtraction-Rings* are engraven *inverſo ordine*, as it is ſaid before, you will find that it is the ſame as in *Addition*.

III. *Multiplication.* For inſtance; if your are to multiply 43 by 3, bring the *o* in all your *Addition-Rings* to the Indexes, as alſo in all your *Multiplication-Plates* in the Windows. Write down (which is more particularly neceſſary if the Numbers are larger than here) the Multiplicand 43 upon the Ivory-Plates near the two Determinators in the two Syſtems BB and CC: But the Multiplicator 3, you may write only on the Ivory-Plate under the Window of the Syſtem BB. Set the Determinators at 43; then move your Operators ſucceſſively as often up and down, till there appears in both Windows the Number 3; then

then you will ſee on your *Addition-Rings* under the Indexes, the Product 129.

It is eaſy to underſtand, that as the *Multiplication* is nothing elſe than a repeated *Addition*, the Machine does alſo perform it's Operation by a repeated *Addition* only: For the Number 3, which appears in the Window of the Syſtem B B, ſhews how many Times you have added the Number 3, pointed by the Determinator to itſelf, which when done 3 Times, is 9. And ſo the ſame Number 3, which appears in the Window of the Syſtem C C, after your Operation, ſhews how many Times you have added the Number 4 to itſelf. I need not to make you obſerve, that beſides the two Syſtems B B and C C, there muſt be ſuppoſed another, not expreſs'd in the Figure, which will ſhew the Number 1 of the Product 129.

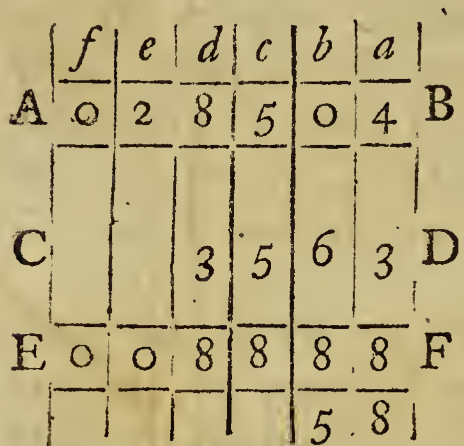
IV. *Diviſion.* If you are, for inſtance, to divide 40 by 3, ſet your Dividend 40 in the *Subſtraction-Rings* under the Indexes *yy*, in the Syſtem B B and C C; turn the Indexes *ff*, *ff*, near the Windows to make *o* appear; write your Diviſor near the Determinator of the Syſtem C C, and ſet the Determinator at 3; pull the Operator up and down, then you will have 1 under the Index *y*, and 1 likewiſe in the Window. By this you ſee, that you cannot work further in this Syſtem C C, becauſe you cannot ſubſtract 3 from 1: You muſt therefore go on, to the other Figure of the Dividend, *viz.* 0, and in the Syſtem B B ſet the Determinator again at 3. This being done, the firſt pulling of the Operator up and down will produce 1 in the Window, and 7 in the *Subſtraction-Ring* under the Index, and the Number 1 which remained before in the Syſtem C C will be changed into *o*. Now as 7 is more than 3, you muſt work on accordingly; having done it twice more, you will find that there remains under the Index *y* but 1, (which is the Numerator of your Fraction) and below in the two Windows the Quotient 13. When you conſider that *Diviſion* is nothing elſe but a repeated *Subſtraction*, you will alſo eaſily underſtand the Reaſon of this Operation.

Thoſe that underſtand the Matter ever ſo little, may now eaſily conceive how they are to proceed with this Machine in larger *Examples*: However, for greater Clearneſs, I will explain it by two *Examples*.

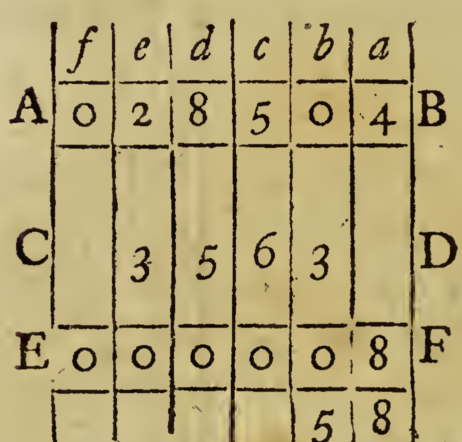
Suppoſing there are ſix Syſtems, *a*, *b*, *c*, *d*, *e*, *f*; Let all the Numbers pointed to by the Indexes *ww* be in A B; thoſe which are to be pointed to by the Determinators in C D; and thoſe which are ſeen in the Windows, in E F. Firſt of all, you muſt turn all your *Addition-Rings* of the *Silver-Plates* and your *Multiplication-Plates* to *o*; *viz.* that under all the Indexes *ww*, and in the Windows nothing may appear but *o*. Write the Number 3563 near the Determinator, in the Syſtems *a*, *b*, *c*, *d*, and direct them accordingly:

	f	e	d	c	b	a	
A	o	o	o	o	o	o	B
C			3	5	6	3	D
E	o	o	o	o	o	o	F
					5	8	

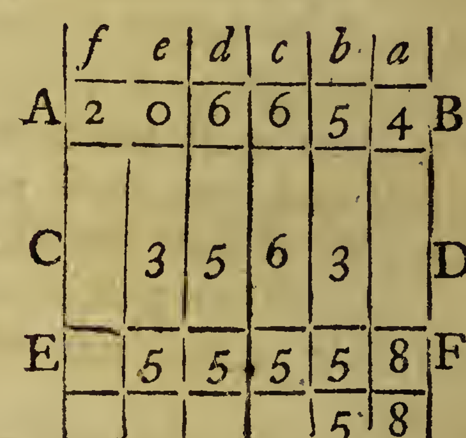
cordingly: The other Number 58, you muſt write down likewiſe, but under the Windows in Syſtem *a* and *b*, as you ſee in this Scheme. Move the ſeveral Operators, which are moveable, ſucceſſively as often up and down, till 8 appears below in the Windows, and you will have under the Indexes above 28504, the Product of 3563 × 8. And ſo the Numbers of the Machine will appear thus.

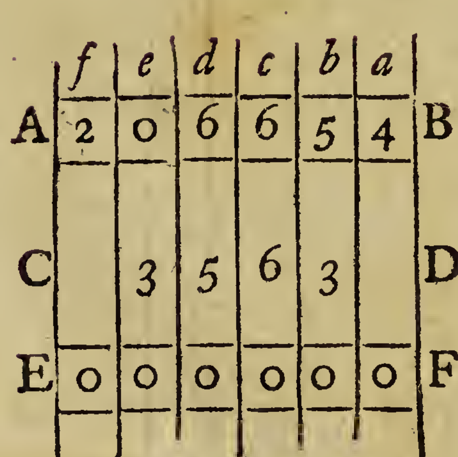


Next advance your Multiplicand 3563, from the Right to the Left; that is to ſay, place the Determinator in the Syſtem *b* at 3, in *c* at 6, in *d* at 5, in *e* at 3, and reduce every Number in the Windows to 0, except in the Syſtem *a*. See the *Scheme* following.



Then pull all the Operators again ſucceſſively in *b*, *c*, *d*, and *e*, up and down, till 5 appears in the Windows below, and you will find at laſt under the Indexes 206654, the Product of 3563 × 58.





But if you are to divide again 206654 by 3563, you muſt place the Dividend above in the *Subſtraction-Rings* under the Indexes. In the Windows below, every Figure muſt be 0, likewiſe as in the *Multiplication*; and write the Diviſor under the Dividend, according to *Vulgar Arithmetick*, and as in the *Figure* here annexed.

If you direct the Determinators in *e*, *d*, *c*, *b*, to their Numbers, and ſubſtract this Diviſor by pulling up and down the Operators as often as you can, you will have in the Windows in *e*, *d*, *c*, *b*, every where 5; but on the *Silver-Plates* there will remain 28504. Now advancing

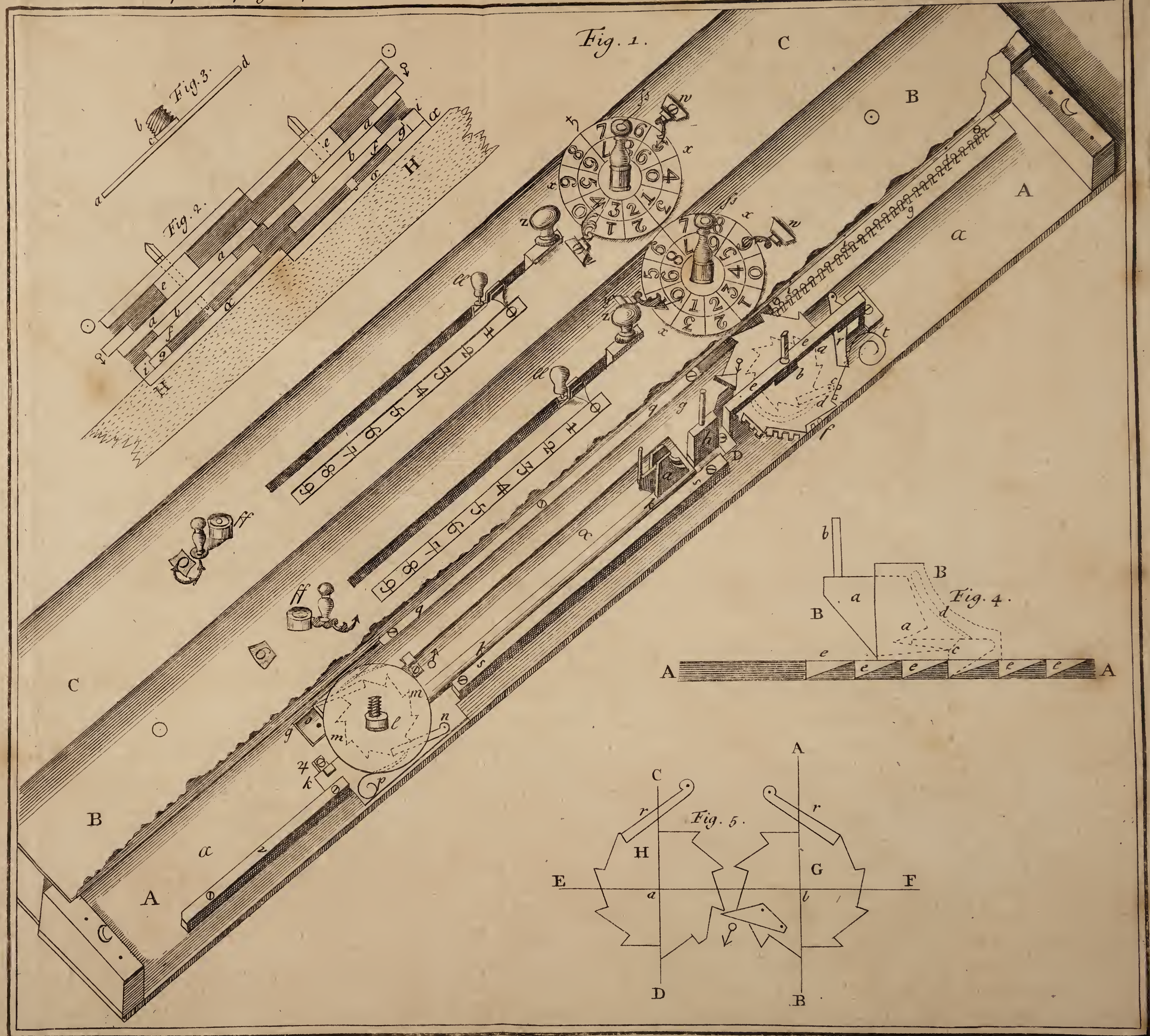


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.

vancing your Diviſor from the Left to the Right, bringing to the Windows in *d*, *c*, *b*, all the Cyphers o, and operating as before, there will at laſt appear on the *Silver-Plates* nothing at all, but below in the Windows 5888. See the *Figure* following:

	f	*e*	*d*	*c*	*b*	*a*	
A	o	o	o	o	o	o	B
C			3	5	6	5	D
E		5	8	8	8	8	F

And here you have only this to obſerve, that in ſuch Caſes, you cut off all the hindermoſt Figures or Numbers in EF, except that which ſtands under the firſt Figure of the Diviſor; what remains is your Quotient.

As for what remains, if it be objected that this Machine cannot be fitted for ſo many and long Numbers, as one would pleaſe, becauſe the Multiplication of ſo many Syſtems would require too great a Force for one Operator to move ſo many Wheels, kept by Springs, ſuppoſing the Caſe that all the Teeth of Communication ſhould duly catch; I own that this Objection is but too well grounded: However, I cannot help obſerving at the ſame Time, that this Defect can hardly be avoided, in any Arithmetical Machine, for performing all thoſe Operations of itſelf, without the Help of the Mind: For there muſt certainly be a particular Syſtem for each Place of Figures, which is to communicate with the next; conſequently, as the Syſtems increaſe in Number, the Force muſt increaſe alſo which is required for moving them all. Beſides, it ought to be conſidered, of what Size ſuch a Machine ought to be, which might ſerve for common Uſe. I think few Calculations could be required, for which 14 or 16 Syſtems might not ſuffice. That which I made was of 7 Syſtems, as I have already mentioned. The Diſpoſition of it was neither ſo well contrived as I have explained it here, nor were it's ſeveral Parts ſo well wrought, as a good Artificer, who makes Profeſſion of ſuch Work, might have performed it; yet thoſe 7 Syſtems were very eaſily put in Motion; and if in a Machine for 14 Figures made by a ſkilful Hand, it could not be ſo eaſily practicable, this Defect, I believe, might be eaſily remedied, by applying the other Hand in the fifth or ſixth Syſtem to the Handle *f s*, in order to eaſe and aſſiſt the Operator.

A brief Account by Mr. John Eames, *F. R. S. of a Work entitled*, The Method of Fluxions and Infinite Series, with it's Application to the

IV. This Text may very well be divided into three Parts: An Introduction, containing the Method of Infinite Series; the Method of Fluxions and Fluents; and laſtly, the Application of both to the moſt conſiderable Problems of the higher Geometry. The Comment conſiſts of very valuable and curious Annotations, Illuſtrations, and Supplements, in order to make the whole a *compleat Inſtitution* for the Uſe of Learners. I ſhall take a kind of comparative View of the Text and Comment together.

Geometry of Curve Lines, *by the Inventor Sir* I. Newton, *Kt. &c. Tranflated from the Author's Latin Original not yet made publick. To which is fubjoined a perpetual Comment upon the whole, &c. by* John Colfon, *M. A.* and *F R. S.* No. 443. *p.* 320. *Oct.* 1736.

The great Author, in what is called the Introduction, teaches the Rudiments of his Method of Infinite Converging Series, which is preparatory to that of Fluxions. In this he fhews how all Compound Algebraical Quantities may be refolved into Series of fimple Terms, which will converge to thofe compound Quantities, or rather to their Roots; juft as in common Decimal Arithmetick, any complicate Number whatever, rational or furd, may be profecuted and exhibited to what Degree of Accuracy we pleafe, by decimal Parts continued *in infinitum.* And this general Arithmetick is here applied to the finding of the Roots of all kinds of Algebraical Equations, whether pure or affected.

And this Doctrine is carried on ftill farther by Mr *Colfon* in his Comment. He purfues the Author's Hint, that vulgar Arithmetick and Algebra, decimal Fractions and infinite Series, have the fame common Foundation, and compofe together but one uniform Science of Computation. For, as in our vulgar Arithmetick, when rightly explained, we exprefs and compute all Numbers by the Root *Ten*, and it's feveral Powers and their Reciprocals, together with a Set of certain known and fmall Coëfficients; fo in this more univerfal Arithmetick of infinite Series, we do the fame thing in effect, by means of any Root affumed at Pleafure, it's Powers and their Reciprocals, difpofed in a regular defcending Order, together with any Coëfficients, as it may happen. And when thefe Series duly converge, they will as truly exhibit by their Aggregate the Quantity required, as a Decimal Fraction infinitely continued will approximate to it's proper *Quæfitum.* This gives him Occafion to expatiate largely upon the Nature and Conftruction of Arithmetical Scales, particular and general; and to inquire into the Nature and Formation of Infinite Series, and their Circumftances of Convergency and Divergency. To explain which he fhews, that in every Series there is always a Supplement to be underftood, when it is not exhibited. This Supplement fums up the Series, and makes it ftop at a finite Number of Terms, in Series that either converge or diverge. Whence in diverging Series it muft neceffarily be found and admitted, or otherwife the Conclufion will not be true; but in converging Series, where it can feldom be known, it may fafely be omitted, becaufe it continually diminifhes with the Terms of the Series, and finally becomes lefs than any affignable Quantity.

The Nature of infinite Series being thus difplayed, he applies them to the Refolution of all kinds of Algebraical Equations. He explains in a very general Manner, the Author's famous Artifice, for finding the *Forms of the Series* for the Roots, and their initial Approximations, by means of a Parallelogram and Ruler, and fhews it's Application in all Cafes. Then he invents many ways of Analyfis, by which the Roots are further profecuted, and may be produced to any Degree of Accuracy required. Alfo many other Speculations are added, to compleat the Doctrine of Series; particularly a very general and ufeful Theorem, for the Solution of all affected Equations in Numbers.

From

From the Refolution of Equations, and the Doctrine of Infinite Series, which finifhes the firft Part of this Work, Sir *I. Newton* proceeds to lay down the Principles of his Method of Fluxions, which is the chief Defign of the prefent Treatife. This Method he founds upon the abftract or rational Mechanicks, by fuppofing all Mathematical Quantities to be generated, as it were, by local Motion, and therefore to have relative Velocities of Increafe or Decreafe, which Velocities he calls *Fluxions.* And the Quantities fo generated by a continual Flux he calls *Fluents* or flowing Quantities; the Relation of which Fluents is always expreffed by fome Algebraical Equation, either given or required. If this Equation be given, and the Relation of the Fluxions is required, it conftitutes the *direct Method of Fluxions*; but when the contrary, 'tis the *inverfe Method of Fluxions.*

Sir *Ifaac*, in his firft Problem, which takes in the direct Method of Fluxions, fhews how to find the Relation of the Fluxions in a very general Manner, and by a great Variety of Solutions. This way of refolving the Problem is peculiar to this Work. He likewife extends it to Equations involving feveral Fluents, which accommodates it to thofe Cafes, wherein any complex or irrational Quantities may be found, or Quantities that are geometrically irreductible. Then he demonftrates the Principles of his Method, or the Precepts of Solution, from the Nature of Moments or vanifhing Quantities, and from the obvious Properties of Equations, which involve indeterminate Quantities.

The Commentator much enlarges upon this whole Doctrine; he enters into the Reafon and Ufe of this Multiplicity of Solutions, and fhews it is a neceffary Refult from the different Forms the fame given Equation may acquire. But efpecially he takes the Author's Demonftration into ftrict Examination, endeavours farther to illuftrate and enforce it's Evidence, and to clear it from all the Objections that either have or may be urged againft it. He even contends, that though the Moments and vanifhing Quantities of the Author could be proved to be impoffible, as has been fuggefted by fome Mathematicians, yet even then they would be fufficient for all the Purpofes of Fluxions, and he produces Inftances of a like Nature from other Parts of Mathematicks. And though the Author, Sir *I. Newton*, in his prefent Treatife, does not directly mention fecond Fluxions, or thofe of higher Orders; yet the ingenious Commentator thinks proper to extend his Enquiries to thefe Orders of Fluxions, demonftrates their Theory, gives Rules and Examples for deriving their Equations, proves their relative Nature, and even exhibits them to View by Geometrical Figures. This laft he does chiefly in what he calls the Geometrical and Mechanical Elements of Fluxions; and he contrives a very general Method, by means of Curve-lines and their Tangents, to make Fluxions and Fluents the Objects of Senfe and ocular Infpection; and thereby

thereby he illuſtrates and verifies the received Methods of deriving their Equations in all Caſes.

In the Author's ſecond Problem, or the Relation of the Fluxions being given to determine the Relation of the Fluents, which includes the inverſe Method of Fluxions, he begins with a particular Solution of it. He calls this Solution particular, becauſe it extends only to ſuch Caſes, wherein the given Fluxional Equation either has been, or might have been, derived from ſome previous finite Algebraical Equation. Then he ſhews how we may return directly to this Equation. But this is ſeldom the Caſe of ſuch Fluxional Equations, whoſe Fluents or Roots are propoſed to be found. For they have commonly Terms either redundant or deficient, by which they cannot be brought under this particular Solution. Therefore to anſwer this Caſe alſo, he gives us a general Solution, in which he extracts the Roots of any propoſed Fluxional Equation, by ſeveral ingenious Methods of Analyſis. And here it is chiefly, that he calls his Method of Infinite Series to his Aſſiſtance; for the Fluent, or Root, will here always be exhibited by a Series. And to find the Fluent in finite Terms, when it can be done, requires particular Expedients, as we ſhall ſee afterwards.

Mr *Colſon*, in his Comment upon this Part of the Work, is very full and explicit. He explains and applies the Author's particular Solution; but is much more copious in explaining the Examples, and clearing up the Difficulties and Anomalies of the general Solution. This is chiefly performed by introducing ſeveral new and ſimple Methods of Analyſis, or Proceſſes of Reſolution; and by applying the Author's Artifice of the Ruler and Parallelogram mentioned before, to theſe Fluxional Equations: By which means not only the Forms of the Series are determined, and their initial Approximations, as has been obſerved above; but likewiſe all the Series may be found, that can be derived from the ſame Fluxional Equation. The Commentator concludes by giving us a very general Method for reſolving all Equations, whether Algebraical or Fluxional; which Method requires no foreign Aſſiſtance, or no ſubſidiary Operations, which all other Methods do. It is founded upon the Uſe and Admiſſion of the higher Orders of Fluxions, and is exemplified by the Solution of ſeveral uſeful Problems. Here the Comment leaves us, but we will go on with our Author.

Having thus taught us the Method of Fluxions both direct and inverſe, he proceeds to apply this Method to ſome very curious and general Problems, chiefly in the Geometry of Curve-lines. As firſt, he determines the *maxima* and *minima* of Quantities in all Caſes, and propoſes ſome elegant Problems to illuſtrate this Doctrine. Then he teaches us to draw Tangents to Curves, whether Geometrical or Mechanical, and that after a great Variety of Ways, or however the Nature of the Curve may be defined. Here likewiſe he propoſes ſome Queſtions, to exerciſe and improve the Learner: Then is very particular upon finding the Quantity of Curvature, at any Point of a given Curve,

Curve, whether Geometrical or Mechanical, or in determining the Centre and the Radius of Curvature: To which ſeveral other curious Speculations are ſubjoined of a like Nature. Here he communicates a very elegant and entirely new Problem, for determining the Quality of the Curvature, at any Point of a given Curve; or how the Curvature proceeds in reſpect of it's greater or leſs Inequability.

Afterwards he goes on to the Quadrature of Curves, which chiefly gives Occaſion to apply the inverſe Method of Fluxions; and firſt he ſhews how, by the direct Method, to find as many Curves as you pleaſe, (or to determine their Equations) the Areas of which ſhall be capable of an exact Quadrature. Then he ſhews how to find as many Curves as you pleaſe, which, though not capable of a juſt Quadrature, yet their Areas may be compared to thoſe of the Conic Sections, or of ſuch other Curves as ſhall be aſſigned. Laſtly, He ſhews how to determine in general the Area of any Curve that ſhall be propoſed, chiefly by the Method of Infinite Series; where many curious and uſeful Speculations are occaſionally introduced and inſerted: As how to aſcertain the Limits of an Area, when thus found analytically; how commodiouſly to ſquare the Circle, the Ellipſis, or Hyperbola, and how to apply the Quadrature of this laſt to the computing a Canon of Logarithms; the Conſtruction of Tables for the ready finding of Quadratures, or the Compariſon of Areas, and how to apply them to the ſolving of other like Problems; the forming of Conſtructions, and demonſtrating Theorems by Fluxions; the approximating to Areas mechanically, and ſuch like.

From finding of Areas he proceeds to the *Rectification of Curves*; and firſt he ſhews how to find as many Curves as you pleaſe, whoſe Curve-lines are capable of an exact Rectification. Then he teaches us to find as many Curves as we pleaſe, whoſe Curve-lines, though not capable of a juſt Rectification, yet may be compared with the Lengths of any Curve-lines aſſigned, or with the Areas of any Curve, when reduced to the Order of Lines. Laſtly, he determines the Lengths of any Curve in general, and gives ſeveral proper Examples of it. All which elegant Speculations are managed with admirable Skill, great Subtility, and fine Contrivance.

V. In the firſt Book, he conſiders the Properties of the three Sections of a Cone, as well in, as out of the Cone. And to make this Part of the Work of more Service to the Reader, he has not only ſelected the moſt conſiderable Properties of theſe Curves that are to be met with in other Writers, both antient and modern, but has added ſeveral new ones, which, as he informs us, are inſerted in their proper Places. And that ſuch Gentlemen as are deſirous to read Sir *I. Newton*'s *Principia*, but are a Loſs for want of a ſufficient Acquaintance with Conic-Sections, may be the more obliged, he has taken particular Care to demonſtrate ſuch Properties as Sir *Iſaac* preſuppoſes his Reader to be acquainted withal. Accordingly, he has prefixed a Table of ſuch Propo-

An Account by Mr John Eames, *F.R.S. of a Book entituled;* A Mathematical Treatiſe, containing a Syſtem of Conic-Sections, with the Doctrine of Fluxions and Fluents, applied to va-

rious Subjects. *By* John Muller. No. 446. p. 87. *July*, &c. 1737.

Propofitions, informing him as well where they are to be met with in this Book, as in Sir *I. Newton*'s *Principia Mathematica.*

The Proofs made ufe of in his Demonftrations, are fometimes Algebraical, at other Times Geometrical, according as he finds the one to be plainer and fhorter than the other.

Book II.

The fecond Book treats of the direct Method of Fluxions. And here he hopes the firft Principles of this Method are laid down, not only in a new, but very plain and concife Manner. He proceeds to fhew the Ufe of Fluxions in the Solution of the common Problems of finding the *Maxima* and *Minima* of Quantities, the *Radii* of the Evolution of Curves, and the *Radii* of Refraction and Reflection. Under the firft of thefe Heads he tells us, particular Care has been taken to diftinguifh the *Maximums* from the *Minimums*, a Thing which has not been taken Notice of fo much as it ought to have been. And whereas fome Mathematicians having made ufe of what they call infinitely fmall Quantities, are forced to reject fomething out of the Equation, for finding the Fluxion of a Rectangle, whofe Sides are varying Quantities, Mr *Muller* ufes only finite Quantities; and finds the Fluxion of fuch a Rectangle after a new Manner, without rejecting any Quantity for it's Smallnefs. He does the fame in finding the Fluxion of a Power. And to avoid the Ufe of infinitely fmall Quantities, introduces a new Principle, *viz.* That a Curve-Line may be confidered as generated by the Motion of a Point carried along by two Forces or Motions, one in a Direction always parallel to the Abfcifs, and the other in a Direction always parallel to the Ordinate. Hence he infers, that the Fluxion of the Ordinate is to the Fluxion of the Abfcifs, as the Ordinate is to the Subtangent of the Curve.

Having likewife proved from the firft Suppofition, that if the defcribing Point, when arrived at any Place given, fhould continue to move onwards, with the Velocity it has there, it would proceed in a right Line, which would touch the Curve in that Point; he concludes that the Direction of the Force in that Place is in the Tangent to the Curve: Confequently, the three Directions being known in each Place, the Proportion between the Velocities of the urging Forces will be likewife known. So that the Nature of the Curve being given, the Law obferved by thefe Velocities may be found; and if the Law of the Velocities be given, the Nature of the Curve may likewife be given.

Book III.

In the third and laft Book, we have the inverfe Method of Fluxions, with it's Application to the feveral Problems folvable by it; fuch as the fuperficial and folid Contents of curvilineal Figures, the Rectification of Curve-Lines, Centers of Gravity, Ofcillation and Percuffion. Here alfo Mr *Cotes*'s Table of Fluents are explained and illuftrated by Examples.

He finifhes this Book with a great Variety of Problems, that are of a Phyfico-Mathematical Nature, feveral of which are new, and propofed to him by Mr *Belidor*. Some, indeed, are not fo, having been

ſolved by Meſſieurs *Varignon* and *Parent*; but then he has ſolved them after a different, and, as he hopes, a more agreeable, Manner, the Conſtruction being more ſimple, and the Proceſs much ſhorter.

V. The Author's firſt Deſign in compoſing this Treatiſe, was to eſtabliſh the Method of Fluxions on Principles equally evident and unexceptionable with thoſe of the antient Geometricians, by Demonſtrations deduced after their Manner, in the moſt rigid Form, and by illuſtrating the more abſtruſe Parts of the Doctrine, to vindicate it from the Imputation of Uncertainty or Obſcurity. But he has likewiſe comprehended in this Work the Application of Fluxions to the moſt important geometrical and philoſophical Enquiries. It conſiſts of an Introduction, and two Books. In the Introduction he gives an Abſtract of the Diſcoveries of the Antients in the higher Parts of Geometry, with Obſervations on their Method, and thoſe that firſt ſucceeded to it. The firſt Book treats of Fluxions in a geometrical Method, and the ſecond treats of the Computations.

An Account of a Book entitled, A Treatiſe of Fluxions, in Two Books, *by* Colin McLaurin, *A. M. Prof. Math.* Edinb. *F. R. S.* 4to. *in 2 Vol. pag.* 763. No. 468. p. 325. *Preſented* Jan. 27, 1742 3.

In the Introduction we have an Abſtract not only of the Diſcoveries of the Antients in the higher Parts of Geometry, but likewiſe of their Demonſtrations. After an Account of the Propoſitions of this Kind, that are to be found in the 12th Book of *Euclid*, there follows a Summary of what is moſt material in the Treatiſes of *Archimedes*, concerning the Sphere and Cylinder, Conoids and Spheroids, the Quadrature of the *Parabola* and the ſpiral Lines. The Demonſtrations are not preciſely in the ſame Form as thoſe of *Archimedes*, but are often illuſtrated from the elementary Propoſitions concerning the Cone, or Corollaries from them, after the Example of *Pappus*, from whom the Propoſition is demonſtrated, and rendered more general, concerning the Area of the Spiral that is generated on a ſpherical Surface by the Compoſition of two uniform Motions analogous to thoſe by which the Spiral of *Archimedes* is deſcribed on a Plane. This Area, though a Portion of a curve Surface, is found to admit of a perfect Quadrature, and this Propoſition concludes the Abſtract. He takes Occaſion from theſe Theorems to demonſtrate ſome Properties of the Conic Sections, that are not mentioned by the Writers on that Subject; and there are more of this Kind deſcribed in Chap. 11 and 14 of Book I.

Coll. Math. Prop. 21. Lib. 4.

It is known, that if a Parallelogram, circumſcribed about a given Ellipſe, have it's Sides parallel to the conjugate Diameters, then ſhall it's Area be of an invariable or given Magnitude, and equal to the Rectangle contained by the Axis of the Figure; but this is only a Caſe of a more general Propoſition. For if, upon any Diameter produced without the Ellipſe, you take two Points, one on each Side of the Center at equal Diſtances from it, and the Four Tangents be drawn from theſe Points to the Ellipſe, thoſe Tangents ſhall form a Parallelogram, which is always of a given or invariable Magnitude, when the Ellipſe is given, if the *Ratio* of thoſe Diſtances to the Diameter be given; and when the *Ratio* of thoſe Diſtances to the Semidiameter is that of the Diagonal

of a Square to the Side, (or of $\sqrt{2 \text{ to } 1}$) the Parallelogram has it's Sides parallel to conjugate Diameters. It is likewife fhown here, how theTriangles, *Trapezia*, or Polygons of any Kind are determined, which circumfcribed about a given Ellipfe, are always of a given Magnitude.

There is alfo a general Theorem concerning the *Fruftum* of a Sphere, Cone, Spheroid, or Conoid, terminated by parallel Planes, when compared with a Cylinder of the fame Altitude on a Bafe equal to the middle Section of the *Fruftum* made by a Parallel Plane. The Difference betwixt the *Fruftum* and the Cylinder is always the fame in different Parts of the fame, or of fimilar Solids, when the Inclination of the Planes to the Axis, and the Altitude of the *Fruftum*, are given. This Difference vanifhes in the parabolic Conoid. It is the fame in all Spheres; being equal to half the Content of a Sphere of a Diameter equal to the Altitude of the *Fruftum*. In the Cone it is one Fourth of the Content of a fimilar Cone of the fame Height with the *Fruftum*; and in other Figures it is reduced to the Difference in the Cone.

In the Remarks on the Method of the Antients, the Author obferves, that they eftablifhed the higher Parts of their Geometry on the fame Principles as the Elements of the Science, by Demonftrations of the fame Kind; that they feem to have been careful not to fuppofe any thing to be done, till by a previous Problem they had fhewn how it was to be performed: Far lefs did they fuppofe any thing to be done, that cannot be conceived to be poffible, as a Line or Series to be actually continued to Infinity, or a Magnitude to be diminifhed till it become infinitely lefs than it was. The Elements into which the refolved Magnitudes were always finite, and fuch as might be conceived to be real. Unbounded Liberties have been introduced of late, by which Geometry (wherein every thing ought to be clear) is filled with Myfteries, and Philofophy is likewife perplexed. Several Inftances of this Kind are mentioned. The Series 1, 2, 3, 4, 5, 6, 7, &c. is fuppofed by fome to be actually continued to Infinity; and, after fuch a Suppofition, we are puzzled with the Queftion, Whether the Number of finite Terms in fuch a Series is finite or infinite. In order to avoid fuch Suppofitions, and their Confequences, the Author chofe to follow the Antients in their Method of Demonftration as much as poffible. Geometry as been always confidered as our fureft Bulwark againft the Subtleties of the Scepticks, who are ready to make ufe of any Advantages that may be given them againft it *; and it is important, not only that the Conclufions in Geometry be true, but likewife that their Evidence be unexceptionable. However, he is far from affirming, that the Method of Infinitefimals is

* See *Bayle*'s Dictionary, Article *Zeno.*

without

without Foundation, and afterwards endeavours to juſtify a proper Application of it.

The Grounds of the Method of Fluxions are deſcribed in Chap. 1. Book I. and again Chap. 1. Book II. In the former, Magnitudes are conceived to be generated by Motion, and the Velocity of the generating Motion is the Fluxion of the Magnitude. Lines are ſuppoſed to be generated by the Motion of Points. The Velocity of the Point that deſcribes the Line is it's Fluxion, and meaſures the Rate of it's Increaſe or Decreaſe. Other Magnitudes may be repreſented by Lines that increaſe or decreaſe in the ſame Proportion with them; and their Fluxions will be in the ſame Proportion as the Fluxions of thoſe Lines, or the Velocities of the Points that deſcribe them. When the Motion of a Point is uniform, it's Velocity is conſtant, and is meaſured by the Space which is deſcribed by it in a given Time. When the Motion varies, the Velocity at any Term of the Time is meaſured by the Space which would be deſcribed in a given Time, if the Motion was to be continued uniformly from that Term without any Variation. In order to determine that Space, and conſequently the Velocity which is meaſured by it, four Axioms are propoſed concerning variable Motions, two concerning Motions that are accelerated, and two concerning ſuch as are retarded. The firſt is, That the Space deſcribed by an accelerated Motion is greater than the Space which would have been deſcribed in the ſame Time, if it had not been accelerated, but had continued uniform from the Beginning of the Time. The ſecond is, That the Space which is deſcribed by an accelerated Motion, is leſs than the Space which is deſcribed in an equal Time by the Motion which is acquired by that Acceleration continued afterwards uniformly. By theſe, and two ſimilar Axioms concerning retarded Motions, the Theory of Motion is rendered applicable to this Doctrine with the greateſt Evidence, without ſuppoſing Quantities infinitely little, or having Recourſe to prime or ultimate *Ratios.* The Author firſt demonſtrates from them all the general Theorems concerning Motion, that are of Uſe in this Doctrine; as that when the Spaces deſcribed by two variable Motions are always equal, or in a given *Ratio*, the Velocities are always equal, or in the ſame given *Ratio*; and converſely, when the Velocities of two Motions are always equal to each other, or in a given *Ratio*, the Spaces deſcribed by thoſe Motions in the ſame Time are always equal, or in that given *Ratio*; that when a Space is always equal to the Sum or Difference of the Spaces deſcribed by two other Motions, the Velocity of the firſt Motion is always equal to the Sum or Difference of the Velocities of the other Motions; and converſely, that when a Velocity is always equal to the Sum or Difference of two other Velocities, the Space deſcribed by the firſt Motion is always equal to the Sum or Difference of the Spaces deſcribed by theſe two other Motions. In comparing Motions in this Doctrine, it is convenient and

 uſual

ufual to fuppofe one of them uniform; and it is here demonftrated, that if the Relation of the Quantities be always determined by the fame Rule or Equation, the *Ratio* of the Motions is determined in the fame Manner, when both are fuppofed variable. Thefe Propofitions are demonftrated ftrictly by the fame Method which is carried on in the enfuing Chapters for determining the Fluxions of the Figures.

In Chap. II. a Triangle that has two of it's Sides given in Pofition, is fuppofed to be generated by an Ordinate moving parallel to itfelf along the Bafe. When the Bafe increafes uniformly, the Triangle increafes with an accelerated Motion, becaufe it's fucceffive Increments are *Trapezia*, that continually increafe. Therefore, if the Motion with which the Triangle flows, was continued uniformly from any Term for a given Time, a lefs Space would be defcribed by it than the Increment of the Triangle, which is actually generated in that Time by Axiom I. but a greater Space than the Increment which was actually generated in an equal Time preceding that Term, by Axiom II. and hence it is demonftrated, that the Fluxion of the Triangle is accurately meafured by the Rectangle contained by the correfponding Ordinate of the Triangle, and the right Line which meafures the Fluxion of the Bafe. The Increment which the Triangle acquires in any Time, is refolved into two Parts; that which is generated in confequence of the Motion with which the Triangle flows at the Beginning of the Time, and that which is generated in confequence of the Acceleration of this Motion for the fame Time. The latter is juftly neglected in meafuring that Motion (or the Fluxion of the Triangle at that Term) but may ferve for meafuring it's Acceleration, or the fecond Fluxion of the Triangle. The Motion with which the Triangle flows, is fimilar to that of a Body defcending in free Spaces by an uniform Gravity, the Velocity of which, at any Term of the Time, is not to be meafured by the Space defcribed by the Body in a given Time, either before or after that Term, becaufe the Motion continually increafes, but by a Mean between thefe Spaces.

When the Sides of a Rectangle increafe or decreafe with uniform Motions, it may be always confidered as the Sum or Difference of a Triangle and *Trapezium*; and it's Fluxion is derived from the laft Propofition. If the Sides increafe with uniform Motions, the Rectangle increafes with an accelerated Motion; and in meafuring this Motion at any Term of the Time, a Part of the Increment of the Rectangle, that is here determined, is rejected, as generated in confequence of the Acceleration of that Motion.

The Fluxions of a curvilineal Area (whether it be generated by an Ordinate moving parallel to itfelf, or by a Ray revolving about a given Center) and of the Solid, generated by the Area revolving about the Bafe, are determined by Demonftrations of the fame Kind; and when the Ordinates of the Figure increafe, the Increment of the Area is refolved in like manner into two Parts, one of which is only to be re-

tained in meafuring the Fluxion of the Area, the other being rejected as generated in confequence of the Acceleration of the Motion with which the Figure flows. An Illuftration of fecond and third Fluxions is given by refolving the Increment of a Pyramid or Cone into the feveral refpective Parts that are conceived to be generated in confequence of the firft, fecond, and third Fluxions of the Solid, when the Axis is fuppofed to flow uniformly.

In Chap. V. a Series of Lines in Geometrical Progreffion are reprefented by an eafy Conftruction. The firft Term being fuppofed invariable, and the fecond to increafe uniformly, all the fubfequent Terms increafe with accelerated Motions. The Velocities of the Points that defcribe thofe Lines being compared, it is demonftrated from the Axioms by common Geometry, that the Fluxions of any two Terms are in a *Ratio* compounded of the *Ratio* of the Terms, and of the *Ratio* of the Numbers that exprefs how many Terms precede them in the Progreffion.

In Chap. VI. the Nature and Properties of Logarithms are defcribed after the celebrated Inventor; and it is obferved, that he made ufe of the very Terms *Fluxus* and *Fluat* on this Occafion. A Line is faid to increafe or decreafe *proportionally*, when the Velocity of the Point, that defcribes it, is always as it's Diftance from a certain Term of the Line; and if in the mean time another Point defcribes a Line with a certain uniform Motion, the Space defcribed by the latter Point is always the Logarithm of the Diftance of the former from the given Term. Hence the Fluxion of this Diftance is to the Fluxion of it's Logarithm as that Diftance is to an invariable Line; and the Fluxions of the Quantities that have their Logarithms in an invariable *Ratio*, are to each other in a *Ratio* compounded of this invariable *Ratio*, and of the *Ratio* of the Quantities themfelves. Some Propofitions are demonftrated, that relate to the Computation of Logarithms; but this Subject is profecuted farther in the fecond Book. The Logarithmick Curve is here defcribed, with the Analogy betwixt Logarithms, and Hyperbolic *Ratios*.

In Chap. VII. after a general Definition of Tangents, it is demonftrated, that the Fluxions of the Bafe, Ordinate, and Curve, are in the fame Proportion to each other, as the Sides of a Triangle refpectively parallel to the Bafe, Ordinate, and Tangent. When the Bafe is fuppofed to flow uniformly, if the Curve be convex towards the Bafe, the Ordinate and Curve increafe with accelerated Motions; but their Fluxions at any Term are the fame as if the Point which defcribes the Curve had proceeded uniformly from that Term in the Tangent there. Any further Increment which the Ordinate or Curve acquires, is to be imputed to the Acceleration of the Motions with which they flow. A Ray that revolves about a given Center, being fuppofed to meet any Curve and an Arc of a Circle, defcribed from the fame Center, the Fluxions of the Ray, Curve, and circular Arc, are compared together; and feveral other Propofitions concerning Tangents are demonftrated from the

 Axioms.

Axioms. The next Chapter treats of the Fluxions of curve Surfaces in a ſimilar Manner.

Chap. IX. treats chiefly of the greateſt and leaſt Ordinates of Figures, and of the Points of contrary Flexure and Cuſpids. The Fluxion of the Baſe being given, when the Fluxion of the Ordinate vaniſhes, the Tangent becomes parallel to the Baſe, and the Ordinate moſt commonly is a *Maximum* or *Minimum*, according to the Rule given by Authors upon this Subject. But if the 2d Fluxion of the Ordinate vaniſh at the ſame Time, and the 3d Fluxion be real, this Rule does not hold, for the Ordinate is in that Caſe neither a *Maximum* nor *Minimum*. If the 1ſt, 2d, and 3d Fluxions vaniſh, and the 4th Fluxion be real, the Ordinate is a *Maximum* or *Minimum*. The general Rule demonſtrated in this Chapter, and again in the laſt Chapter of Book II. is, that when the 1ſt Fluxion of the Ordinate, with it's Fluxions of any ſubſequent ſucceſſive Orders, vaniſh, and the Number of all theſe Fluxions that vaniſh is odd, then the Ordinate is a *Maximum* or *Minimum*, according as the Fluxion of the next Order to theſe is negative or poſitive. The Ordinate paſſes through a Point of contrary Flexure, when it's Fluxion becomes a *Maximum* or *Minimum*, ſuppoſing the Curve to be continued on both Sides of the Ordinate. Hence the common Rule for finding the Points of contrary Flexure is corrected in a ſimilar Manner. Such a Point is not always formed when the 2d Fluxion of the Ordinate vaniſhes; for if it's 3d Fluxion likewiſe vaniſhes, and it's 4th Fluxion be real, the Curve may have it's Cavity turned all one Way. The ſame is to be ſaid, when it's Fluxions of the ſubſequent ſucceſſive Orders vaniſh, if the Number of all thoſe that vaniſh be even. Other Theorems are ſubjoined relating to this Subject.

Chap. X. treats of the Aſymptotes of Lines, the Areas bounded by them and the Curves, the Solids generated by theſe Areas of ſpiral Lines, and the Limits of the Sums of Progreſſions. The Analogy there is betwixt theſe Subjects, induced the Author to treat of them in one Chapter, and illuſtrate them by one another. He begins with three of the moſt ſimple Inſtances of Figures that have Aſymptotes. In the common Hyperbola, the Ordinate is reciprocally as the Baſe, and therefore decreaſes while the Baſe increaſes, but never vaniſhes, becauſe the Rectangle contained by it and the Baſe is always a given Area, and it is aſſignable at any aſſignable Diſtance, how great ſoever. The Points of the Conchoid are determined by drawing right Lines from a given Center, and upon theſe produced from the Aſymptote, taking always a given right Line; ſo that the Curve never meets the Aſymptote, but continually approaches to it, becauſe of the greater and greater Obliquity of this right Line. The 3d is the Logarithmic Curve, wherein the Ordinates, at equal Diſtances, decreaſe in Geometrical Proportion, but never vaniſh, becauſe each Ordinate is in a given *Ratio* to the preceding Ordinate. Geometrical Magnitude is always underſtood to conſiſt of

Parts;

Parts; and to have no Parts, or to have no Magnitude, are confidered as equivalent in this Science *. There is, however, no Neceffity for confidering Magnitude as made up of an infinite Number of fmall Parts; it is fufficient, that no Quantity can be fuppofed to be fo fmall, but it may be conceived to be diminifhed further; and it is obvious, that we are not to eftimate the Number of Parts that may be conceived in a given Magnitude, by thofe which in particular determinate Circumftances may be actually perceived in it by Senfe; fince a greater Number of Parts become vifible in it by varying the Circumftances in which it is perceived.

It is hardly poffible to give a tolerable Extract of this or the following Chapters, without Diagrams and Computations: We fhall therefore obferve only, that after giving fome plain and obvious Inftances, wherein a Quantity is always increafing, and yet never amounts to a certain finite Magnitude (as, while the Tangent increafes, the Arc increafes, but never amounts to a Quadrant); this is applied fucceffively to the feveral Subjects mentioned in the Title of the Chapter. Let the Figure be concave towards the the Bafe, and fuppofe it to have an Afymptote parallel to the Bafe; in this Cafe the Ordinate always increafes while the Bafe is produced, but never amounts to the Diftance between the Afymptote and the Bafe. In like manner a curvilineal Area, in a fecond Figure, may increafe, while the Bafe is produced, and approach continually to a certain finite Space, but never amount to it: This is always the Cafe, when the Ordinate of this latter Figure is to a given right Line, as the Fluxion of the Ordinate of the former is to the Fluxion of the Bafe; and of this various Examples are given. A Solid may increafe in the fame Manner, and yet never amount to a given Cube or Cylinder, when the Square of the Ordinate of the latter Figure is to a given Square, as the Fluxion of the Ordinate of the firft Figure is to the Fluxion of the Bafe. A Spiral may in like manner approach to a Point continually, and yet in any Number of Revolutions never arrive at it; and there are Progreffions of Fractions that may be continued at Pleafure, and yet the Sum of the Terms may be always lefs than a given Number. Various Rules are demonftrated, and illuftrated by Examples, for determining when a Figure has an Afymptote parallel or oblique to the Bafe; when the Area terminated by the Curve and the Afymptote has a Limit which it never exceeds, or may be produced till it furpafs any affignable Space; when the Solid generated by that Area, the Surface generated by the Perimeter of the Curve, the fpiral Area generated by the revolving Ray, the fpiral Line itfelf, or the Sum of the Terms of a Progreffion, have fuch Limits or not; and for meafuring thofe Limits. The Author infifts on thefe Subjects, the rather that they are commonly defcribed in very myfterious Terms, and have been the moft fertile of Paradoxes of any

* See *Euclid's* Elements, Def. I. Lib. I.

Parts

Parts of the higher Geometry. These Pradoxes, however, amount to no more than this: That a Line or Number may be continually acquiring Increments, and those Increments may decrease in such a Manner, that the whole Line or Number shall never amount to a given Line or Number. The Necessity of admiting this is obvious enough, and is here shewn from the Nature of the most common geometrical Figures in Art. 292, 293, *&c.* and from any Series of Fractions that decrease continually, in Art. 354, 355, *&c.*

Chap. XI. treats of the Curvature of Lines, it's Variation, the Degrees of Contact of the Curve and Circle of Curvature, and of various Problems that depend on the Curvature of Lines. This Subject is treated fully, because of it's extensive Usefulness, and because in this consists one of the greatest Advantages of the modern Geometry above that of the Antients. The Author on this, as former Occasions, begins by premising the necessary Definitions. Curve Lines touch each other in a Point, when the same right Line is their common Tangent at that Point; and that which has the closest Contact with the Tangent, or passes betwixt it and the other Curve through the Angle of Contact formed by them, being less inflected from the Tangent, is therefore less curve. Thus a greater Circle has a less Curvature than a lesser Circle; and since the Curvature of Circles may be varied indefinitely, by inlarging or diminishing their Diameters, they afford a Scale by which the Curvature of other Lines may be measured. As the Tangent is the right Line which touches the Arc so closely, that no other right Line can be drawn between them; so the Circle of Curvature is that which touches the Curve so closely, that no other Circle can be drawn through the Point of Contact between them. As the Curve is separated from it's Tangent in consequence of it's Flexure or Curvature, so it is separated from the Circle of Curvature in consequence of the Variation of it's Curvature; which is greater or less, according as it's Flexure from that Circle is greater or less.

The Tangent of the Figure being considered as the Base, a new Figure is imagined, whose Ordinate is a third Proportional to the Ordinate and Base of the first. This new Figure determines the Chord of the Circle of Curvature, by it's Intersection with the Ordinate at the Point of Contact, and by the Tangent of the Angle in which it cuts that Circle, measures the Variation of Curvature. The less this Angle is, the closer is the Contact of the Curve and Circle of Curvature, of which there may be indefinite Degrees. When the Figure proposed is a conic Section, the new Figure is likewise a conic Section; and it is a right Line when the first Figure is a *Parabola*, and the Ordinates are parallel to the Axis; or when the first Figure is an *Hyperbola*, and the Ordinates are parallel to either Asymptote. Hence the Curvature and it's Variation in a conic Section are determined by several Constructions; and, amongst other Theorems, it is shewn, that the Variation of Curvature at any Point of a conic Section is as the Tangent of the Angle

Angle contained by the Diameter which paſſes through that Point, and by the Perpendicular to the Curve.

When the Ordinate at the Point of Contact is an Aſymptote to the new Figure, the Curvature is leſs than in any Circle; and this is the Caſe in which it is ſaid to be infinitely little, or the Ray of Curvature is ſaid to be infinitely great. Of this Kind is the Curvature at the Points of contrary Flexure in the Lines of the third Order. When the new Figure paſſes through the Point of Contact, the Curvature is greater than in any Circle, or the Ray of Curvature vaniſhes; and in this Caſe the Curvature is ſaid to be infinitely great. Of this Kind is the Curvature at the Cuſpids of the Lines of the third Order.

As Lines which paſs through the ſame Point have the ſame Tangent when the firſt Fluxions of the Ordinate are equal, ſo they have the ſame Curvature when the ſecond Fluxions of the Ordinate are likewiſe equal; and half the Chord of the Circle of Curvature that is intercepted between the Points wherein it interſects the Ordinate, is a third Proportional to the right Lines that meaſure the ſecond Fluxion of the Ordinate and firſt Fluxion of the Curve, the Baſe being ſuppoſed to flow uniformly. When a Ray revolving about a given Point, and terminated by the Curve, becomes perpendicular to it, the firſt Fluxion of the Ray vaniſhes; and if it's ſecond Fluxion vaniſhes at the ſame time, that Point muſt be the Center of Curvature. The ſame is to be ſaid when the angular Motion of the Ray about that Point is equal to the angular Motion of the Tangent of the Curve; as the angular Motion of the *Radius* of a Circle about it's Center is always equal to the angular Motion of the Tangent of the Circle. Thus the various Properties of the Circle ſuggeſt various Theorems for determining the Center of the Curvature.

Becauſe Figures are often ſuppoſed to be deſcribed by the Interſections of right Lines revolving about given Poles, three Theorems are given in Prop. 18. 26. and 35. for determining the Tangents, Aſymptotes, and Curvature of ſuch Lines, from the Deſcription, which are illuſtrated by Examples. A new Property of Lines of the third Order is ſubjoined to Prop. 35. The Evolution of Lines is conſidered in Prop. 36. The Tangents of the *Evoluta* are the Rays of Curvature of the Line which is deſcribed by it's Evolution; and the Variation of Curvature in the latter, is meaſured by the *Ratio* of the Ray of Curvature of the former to the Ray of Curvature of the latter.

Sir *I. Newton*, in a Treatiſe lately publiſhed, meaſures the Variation of the Curvature by the *Ratio* of the Fluxion of the Ray of Curvature to the Fluxion of the Curve; and is followed by the Author, to avoid the Perplexity which a Difference in Definitions occaſions to Readers, though he hints (in Art. 386.) that this *Ratio* gives rather the Variation of the Ray of Curvature, and that it might have been proper to have meaſured the Variation of Curvature rather by the *Ratio* of the Fluxion of the Curvature itſelf to the Fluxion of the Curve; ſo

that

that the Curvature being inverfely as the Ray of Curvature, and confequently it's Fluxion as the Fluxion of the Ray itfelf directly, and the Square of the Ray inverfely, it's Variation would have been directly as the Meafure of it, according to Sir *I. Newton*'s Definition, and inverfely as the Square of the Ray of Curvature: According to this Explication, it would have been meafured by the Angle of Contact contained by the Curve and Circle of Curvature, in the fame Manner as the Curvature itfelf is meafured by the Angle of Contact contained by the Curve and Tangent. The Ground of this Remark will better appear from an Example: According to Sir *I. Newton*'s Explication, the Variation of Curvature is uniform in the Logarithmic Spiral, the Fluxion of the Ray of Curvature in this Figure being always in the fame *Ratio* to the Fluxion of the Curve; and yet while the Spiral is produced, though it's Curvature decreafes, it never vanifhes; which muft appear ftrange to fuch as do not attend to the Import of his Definition.——It is eafy, however, to derive one of thefe Meafures of this Variation from the other, and becaufe Sir *I. Newton*'s is (generally fpeaking) affigned by more fimple Expreffions, the Author has the rather conformed to it in this Treatife, but thought it neceffary to give the Caution we have mentioned.

The greateft Part of this Chapter is employed in treating of ufeful Problems, that have a Dependance on the Curvature of Lines. Firft, the Properties of the Cycloid are briefly demonftrated, with the Application of this Doctrine to the Motion of Pendulums, by fhewing that when the Motion of the generating Circle along the Bafe is uniform, and therefore may meafure the Time, the Motion of the Point that defcribes the Cycloid, is fuch as would be acquired by a heavy Body defcending along the cycloidal Arc, the Axis of the Figure being fuppofed perpendicular to the Horizon. In the next place, the Cauftics, by Reflexion and Refraction, are determined. If Perpendiculars be always drawn from the radiating Point to the Tangents of the Curve, and a new Curve be fuppofed to be the *Locus* of the Interfections of the Perpendiculars and Tangents, then the Line, by the Evolution of which that new Curve can be defcribed, is fimilar and fimilarly fituated to the Cauftic by Reflection. The Doctrine of centripetal Forces is treated at Length from Art. 416. to 493.

Firft, a Body is fuppofed to defcend freely by it's Gravity in a vertical Line; and becaufe the Gravity is the Power which accelerates the Motion of the Body, it muft be meafured by the Fluxion of it's Velocity, or the fecond Fluxion of the Space defcribed by it. When the vertical Line is fuppofed to move parallel to itfelf with an uniform Motion, the Body will defcend in it in the fame Manner as before; and the Gravity will be ftill meafured by the fecond Fluxion of the Defcent, or the fecond Fluxion of the Ordinate of the Curve that is traced in this Cafe by the Body on an immoveable Plain, and therefore is as the Square of the Velocity (which is meafured by the Fluxion of the

Curve) directly, and the Chord of the Circle of Curvature that is in the Direction of the Gravity inversely, by a Proposition mentioned above. When the Gravity acts uniformly, and in parallel Lines, the Projectile, in describing any Arc, falls below the Tangent drawn at the Beginning of the Arc, as much as if it had fallen perpendicularly in the Vertical; and the Time being given, the Gravity may be measured by the Space which is the Subtense of the Angle of Contact. In other Cases, when the Gravity varies, or it's Direction changes, it may be measured at any Point by the Subtense of the Angle of Contact, that would have been generated in a given Time, if the Gravity had continued to act uniformly in parallel Lines from that Term, that is, by the Subtense of the Angle of Contact in the Parabola that has it's Diameter in the Direction of the Force, and has the closest Contact with the Curve; which leads us to the same Theorem as before.

In general, let the Gravity (that results from the Composition of any Number of centripetal Forces, which are supposed to act on the Body in one Plane) be resolved into a Force parallel to the Ordinates, and a Force parallel to the Base; then the former shall be measured by the second Fluxion of the Ordinate, and the latter by the second Fluxion of the Base, the Time being supposed to flow uniformly, so that the Velocity of the Body may be measured by the Fluxion of the Curve. When the Trajectory is not in one Plane, the Force is resolved in a similar Manner into three Forces, which are measured by three second Fluxions analogous to them.

Whether the Body move in a Void, or in a Medium that resists it's Motion; the Gravity that results from the Composition of the centripetal Forces which act upon the Body, is always as the Square of it's Velocity directly, and the Chord of the Circle of Curvature that is in the Direction of the Gravity inversely.

When a Body describes any Trajectory in a Void or in a Medium, by a Force directed to one given Center, the Velocity at any Point of the Trajectory is to the Velocity by which a Circle could be described in a Void about the same Center, at the same Distance, by the same Gravity, in the subduplicate *Ratio* of the angular Motion of the Ray drawn always from the Body to the Center, to the angular Motion of the Tangent of the Trajectory: And, if there be no Resistance, the Velocity in the Trajectory at any Point, is the same that would be acquired by the Body, if it was to fall from that Point through one fourth of the Chord of the Circle of Curvature that is in the Direction of the Gravity, and the Gravity at that Point was to be continued uniformly during it's Descent.

If the centripetal Force be inversely as any Power of the Distance whose Exponent is any Number m greater than Unit, there is a certain Velocity (*viz.* that which is to the Velocity in a Circle at the same Distance as $\sqrt{2}$ to $\sqrt{m-1}$) which would be just sufficient to carry off the Body upwards in a vertical Line, so as that it should continue to

afcend for ever, and never return towards the Center. If the Body be projected in any other Direction with the fame Velocity, it will defcribe a Trajectory which is here conftructed: It is a *Parabola* when $m=2$, a Logarithmic Spiral when $m=3$, an Epicycloid when $m=4$, a Circle that paffes through the Center of the Forces when $m=5$, and the *Lemnifcata* when $m=7$. In general, it is conftructed by drawing a Perpendicular from the Center of the Forces to a right Line given in Pofition, and any other Ray to the fame right Line, then increafing or diminifhing the Angle contained by this Ray and the Perpendicular in the given *Ratio* of 2 to the Difference between 3 and m, and increafing or diminifhing the Logarithm of the Ray in the fame given *Ratio.* The Trajectories defcribed in analogous Cafes by centrifugal Forces, are conftructed in a fimilar Manner. Thefe are the Figures in which the Perpendicular, from a given Center on the Tangent, is always as fome Power of the Ray drawn from the fame Center to the Point of Contact, which are afterwards found to arife in the Refolution of the moft fimple Cafes of Problems of various Kinds.

When the Area defcribed about the Center of an Ellipfe is given, the Subtenfe of the Angle of Contact, drawn through one Extremity of the Arc parallel to the Semidiameter drawn to the other Extremity is in a given *Ratio* to this Semidiameter; and therefore, when an Ellipfe is defcribed by a Force directed towards the Center, that Force is always as the Diftance from the Centre. When the Force is directed toward the *Focus*, it is inverfely as the Square of the Diftance. And thefe two Cafes are confidered particularly, becaufe of their Ufefulnefs in the true Theory of Gravity. To illuftrate which, the Laws of centripetal Forces that would caufe a Body to defcend continually toward the Center, or afcend from it, are diftinguifhed from thofe which caufe the Body to approach towards the Center, and recede from it by Turns. A Body approaches from the higher Apfid toward the Center, when it's Velocity is lefs than what is requifite to carry it in a Circle; and if it's Velocity increafe, while it defcends, in a higher Proportion than the Velocities requifite to carry Bodies in Circles about the fame Center, the Velocity in the lower Part of the Curve may exceed the Velocity in a Circle at the fame Diftance, and thereby become fufficient to carry off the Body again. But while the Diftance decreafes, if the Velocities in Circles increafe in the fame or in a higher Proportion, than the Velocity in a Trajectory can increafe, the Body muft either continually approach toward the Center, if it once begin to approach to it, or recede continually from the Center, if it once begin to afcend from it; and this is the Cafe, when the centripetal Force increafes as the Cube of the Diftance decreafes, or in a higher Proportion. But though, in fuch Cafes, the Body approach continually towards the Center, we are not to conclude, that it will always approach to it till it fall into it, or come within any given Diftance; for it is demonftrated afterwards in Art. 879 and 880, that it may approach to the Center for ever, in a

Spiral that never deſcends to a given Circle deſcribed in the ſame Plane, and that it may recede from it for ever in a Spiral that never ariſes to a given Altitude. An Example of each Caſe is given when the centripetal Force is inverſely as the fifth Power of the Diſtance.

When the Trajectory is deſcribed in a *Medium*, let z be to a given Magnitude as the centripetal Force is to the Force by which the ſame Trajectory could be deſcribed in a Void; and if the Area be ſuppoſed to flow uniformly, the Reſiſtance will be in the compound *Ratio* of the Fluxion of z, and of the Fluxion of the Curve; and the Denſity of the Medium (ſuppoſing the Reſiſtance to be in the compound *Ratio* of the Denſity and of the Square of the Velocity) ſhall be as the Fluxion of the Logarithm of z directly, and the Fluxion of the Curve inverſely. Hence, when any Figure that can be deſcribed in a Void by a Force that varies according to any Power of the Diſtance from the Center, is deſcribed in a Medium, the Denſity of the Medium muſt be inverſely as the Tangent of the Figure bounded by a Perpendicular at the Center to the Ray drawn from it to the Point of Contact.

After giving ſome Properties of the Trajectories that are deſcribed by a Body when it gravitates in right Lines perpendicular to a given Surface, and their Application to optical Uſes, the Author proceeds to conſider the Motion of a Body that gravitates towards ſeveral Centers. In ſuch Caſes, that Surface is ſaid to be horizontal, which is always perpendicular to the Direction of the Gravity that reſults from the Compoſition of the ſeveral Forces; and it is ſhewn, that the Velocity which is acquired by deſcending from one horizontal Surface to another, is always the ſame (whether the Body move in right Lines, or in any Curves); the Square of which is meaſured by the Aggregate of ſeveral Areas which have the Diſtances from the reſpective Centers for their Baſes, and right Lines proportional to the Forces at theſe Diſtances for their Ordinates.

The Force which acts upon the Moon is reſolved into a Force perpendicular to the Plane of the Ecliptic, and a Force parallel to it. This laſt is again reſolved into that which is parallel to the Line of the *Syzigies*, and that which is parallel to the Line joining the Quadratures. The firſt meaſures the ſecond Fluxion of the Diſtance of the Moon from that Plane, the ſecond and third meaſure the ſecond Fluxions of her Diſtances from the Line of the Quadratures, and from the Line of the *Syzigies*, reſpectively. Hence a Conſtruction is derived of the Trajectory which would be deſcribed by the Moon about the Earth, in conſequence of their unequal Gravitation towards the Sun, if the Gravity of the Moon towards the Earth was as her Diſtance from it. From this a Computation is deduced of the Motion of the Nodes of the Moon, and of the Variation of the Inclination of the Plane of her Orbit, which we cannot deſcribe here. It is ſufficient to obſerve, that theſe Motions are found to agree nearly with thoſe which have been deduced from other Theories, and from Aſtronomical Obſervations.

G 2 A Fluid

A Fluid being ſuppoſed to gravitate towards two given Centers with equal and invariable Forces, it is ſhewn, that the Figure of the Fluid muſt be that of an oblong Spheroid, and that thoſe two Centers muſt be the *Foci* of the generating Ellipſe. The Nature of the Figure is alſo ſhewn, when the Fluid gravitates towards ſeveral Centers, or when it revolves on it's Axis ; but theſe are mentioned briefly, becauſe ſuch Theories are of little or no Uſe for diſcovering the Figures of the Planets.

In Chap. XII, the Author proceeds to conſider the more conciſe Methods, by which the Fluxions of Quantities are uſually determined, and to deduce general Theorems more immediately applicable to the Reſolution of Geometrical and Philoſophical Problems. In the Method of Infiniteſimals, the Element by which any Quantity increaſes or decreaſes, is ſuppoſed to become infinitely ſmall, and is generally expreſſed by two or more Terms, ſome of which become infinitely leſs than the reſt, and therefore being neglected as of no Importance, the remaining Terms form what is called the *Difference* of the Quantity propoſed. The Terms that are neglected in this Manner are the very ſame which ariſe in conſequence of the Acceleration or Retardation of the generating Motion, during the infinitely ſmall Time in which the Element is generated ; and therefore theſe Differences are in the ſame *Ratio* to each other as the generating Motions or Fluxions. Hence the Concluſions in this Method are accurately true, without even an infinitely ſmall Error, and agree with thoſe that are deduced by the Method of Fluxions.

It is uſual in this Method to conſider a Curve as a Polygon of an infinite Number of Sides, which, being produced, give the Tangents of the Curve, and, by their Inclination to each other, meaſure it's Curvature. But it is neceſſary in ſome Caſes, if we would avoid Error, to reſolve the Element of the Curve into ſeveral infinitely ſmall Parts, or even ſometimes into Infiniteſimals of the ſecond Order ; and Errors that might otherwiſe ariſe in it's Application, may, with due Care, be corrected by a proper Uſe of this Method itſelf, of which ſome Inſtances are given. If we were to ſuppoſe, for Example, the leaſt Arc that can be deſcribed by a Pendulum to coincide with it's Chord, the Time of the Vibration derived from this Suppoſition will be found erroneous ; but by reſolving that Arc into more and more infinitely ſmall Parts, we approach to the true Time in which it is deſcribed. By ſuppoſing the Tangent of the Curve to be the Production of the rectilineal Element of the Curve, the Subtenſe of the Angle of Contact is found equal to the ſecond Difference or Fluxion of the Ordinate ; but in this Inquiry, the Tangent ought to be ſuppoſed to be equally inclined to the two Elements of the Curve that terminate at the Point of Contact ; and then the Subtenſe of the Angle of Contact will be found equal to half the ſecond Difference of the Ordinate, which is it's true Value.

Sir

Sir *I. Newton*, however, inveftigates the Fluxions of Quantities in a more unexceptionable Manner. He firft determines the finite fimultaneous Increments of the Fluents, and, by comparing them, inveftigates the *Ratio* that is the Limit of the various Proportions which they bear to each other, while he fuppofes them to decreafe together till they vanifh. When the generating Motions are variable, the *Ratio* of the fimultaneous Increments that are generated from any Term, is expreffed by feveral Quantities, fome of which arife from the *Ratio* of the generating Motions at that Term, and others from the fubfequent Acceleration or Retardation of thefe Motions. While the Increments are fuppofed to be diminifhed, the former remain invariable, but the latter decreafe continually, and vanifh with the Increments; and hence the Limit of the variable *Ratio* of the Increments (or their ultimate *Ratio*) gives the precife *Ratio* of the generating Motions or Fluxions. Moft of the Propofitions in the preceding Chapters may be more briefly demonftrated by this Method, (of which feveral Examples are given) and the Author makes always ufe of it in the Sequel of this Book.

It is one of the great Advantages of this Method, that it fuggefts general Theorems for the Refolution of Problems, which may be readily applied as there is Occafion for them. Our Author proceeds to treat of thefe, and firft of fuch as relate to the Center of Gravity and it's Motion. In any Syftem of Bodies, the Sum of their Motions, eftimated in a given Direction, is the fame as if all the Bodies were united in their common Center of Gravity. If the Motion of all the Bodies is uniform and rectilineal, the Center of Gravity is either quiefcent, or it's Motion is uniform and rectilineal. When Action is equal to Reaction, the State of the Center of Gravity is never affected by the Collifions of the Bodies, or by their attracting or repelling each other mutually. It is not, however, the Sum of the abfolute Motions of the Bodies that is preferved invariable in confequence of the Equality of the Action and Reaction, as they feem to imagine, who tell us, that this Sum is unalterable by the Collifions of Bodies, and that this follows fo evidently from the Equality of Action and Reaction, that to endeavour to demonftrate it, would ferve only to render it more obfcure. On this Occafion the Author illuftrates an Argument which he had propofed in a Piece that obtained the Prize propofed by the *Royal Academy of Sciences* at *Paris* in 1724, againft the Menfuration of the Forces of Bodies by the Square of the Velocities, fhewing that if this Doctrine was admitted, the fame Power or Agent, exerting the fame Effort, would produce more Force in the fame Body when in a Space carried uniformly forwards, than if the Space was at Reft; or that Springs acting equally on two equal Bodies in fuch a Space, would produce unequal Changes in the Forces of thofe Bodies.

Various Problems concerning the Collifion of Bodies are refolved in a more general Manner than ufual. Mr *Bernouilli* had determined the Motions when the Elafticity is perfect, and one Body ftrikes two equal

Bodies in Directions that form equal Angles with it's Direction; or when there are any Number of Bodies impelled by it on one Side in various Directions, providing equal Bodies be impelled by it on the other Side, in Directions equally inclined to it's own Direction. But the Problem is resolved here without these Limitations; some others of this Kind are subjoined, and this Doctrine is applied for determining the Motions of Bodies that act upon each other while they descend by their Gravity.

The general Principle derived from these Inquiries, is, that if there be no Collision, or sudden Communication of Motion from one Body to another, while they descend together, and in any case, if the Elasticity be perfect, the Sum of the Products, when each Body is multiplied by the Square of the Velocity acquired by it, is the same as if all the Bodies had descended freely from the same respective Altitudes to their several Places; only in collecting that Sum, if any Body is made to ascend, the Product of it multiplied by the Square of it's Velocity is to be subducted: And if the Bodies be supposed to ascend from their Places with the respective Velocities acquired by them, then their common Center of Gravity will rise to the same Level from which it descended. In other Cases, however, the Ascent of the Center of Gravity will be less than it's Descent, but is never greater.

After demonstrating the usual Rule for finding the Center of Oscillation, the Author treats of the Motion of Water issuing from a cylindric Vessel. The Effect of the Gravitation of the whole Mass of Water is considered as threefold. It accelerates, for some time at least, the Motion with which the Water in the Vessel descends; it generates the Excess of the Motion with which the Water issues at the Orifice above the Motion which it had in common with the rest of the Water; and it acts on the Bottom of the Vessel at the same Time. Then supposing the last two Parts of the Force to be in any invariable *Ratio* to each other, when the Diameters of the Base and Orifice are given, he determines by Logarithms the Velocity with which the Water issues at the Orifice; and shews that this Velocity will approach very near to it's utmost Limit, in an exceeding small Time. When the Water is supposed to be supplied in a Cylinder, so as to stand always at the same Altitude above the Orifice, there is an Analogy between the Acceleration of the Motion of the Water that issues at the Orifice, and the Acceleration of a Body that descends by it's Gravity in a Medium which resists in the duplicate *Ratio* of the Velocity. For when the utmost Velocities, or Limits, are equal in those two Cases, the Time in which the issuing Water acquires any lesser Velocity, is to the Time in which the descending Body acquires the same Velocity as the Area of the Orifice to the Area of the Base; and if a cylindric Column be supposed to be erected on the Orifice equal to the Quantity of Water that issues at the Orifice in the former of those Times, the Height of this Column will be to the Space described by the descending Body in the latter Time,

Time, in the fame *Ratio* as the Orifice to the Area of the Bafe. The *Ratio* of the Force that acts on the Bottom of the Veffel to the Force that generates the Motion of the Water iffuing at the Orifice, is deduced from Sir *I. Newton*'s Cataract, and is the fame that follows from the Principle concerning the Equality of the Afcent and Defcent of the Center of Gravity, which was firft applied to this Inquiry by Mr *Daniel Bernouilli* Comment. *Acad. Petrop. Tom.* 2. But there are feveral Precautions to be taken in applying this Doctrine.

After fome other Theorems concerning the Center of Gravity, and feveral Obfervations concerning the Curvature of Lines, and the Angles of Contact; the Author reprefents four general Propofitions in one View, that the Analogy between them may appear. The 1ft gives the Property of the Trajectories that are defcribed by any centripetal Forces, how variable foever thefe Forces, or their Directions, may be. The 2d gives a like general Property of the Lines of fwifteft Defcent. The 3d gives the Property of the Line that is defcribed in lefs Time than any other of an equal Perimeter. And the 4th gives the Property of the Figure that is affumed by a flexible Line or Chain, in confequence of any fuch Forces acting upon it. If we fuppofe a Body to fet out from any Point in the Trajectory, or in the Line of fwifteft Defcent, with the Velocity which it has acquired there, and to move in the right Line which is the Direction of the Gravity, that refults from the Compofition of the centripetal Forces, then fhall it's Velocity, and it's Diftance from the Point where the Perpendicular from the Center of Curvature meets that right Line, flow *proportionally*, *i. e.* the Fluxion of the Velocity (or of the right Line that meafures it) fhall be to the Velocity as the Fluxion of that Diftance is to the Diftance. When the Velocity and Direction of the Motion is the fame in the Line of fwifteft Defcent as in the Trajectory, their Curvature is the fame. Thus in the common Hypothefis of Gravity, the Curvature in the Cycloid, the Line of fwifteft Defcent, is the fame as the *Parabola* defcribed by a Projectile, if the Velocities in thofe Lines be equal, and their Tangents be equally inclined to the Horizon. In order to find the Nature of the *Catenaria* in any Hypothefis of Gravity, fuppofe the Gravity to be increafed or diminifhed in the fame Proportion as the Thicknefs of the Chain varies, and to have it's Direction changed into the oppofite Direction; then imagine a Body to fet out with a juft Velocity from a given Point in the Chain, and to defcribe the Curve. The Tenfion of the Chain at any Point will be always as the Square of the Velocity acquired at that Point, and if a Body be projected with this Velocity in the Direction of the Tangent, the Curvature of the Trajectory defcribed by it will be one half of the Curvature of the Chain at that Point. We muft refer to the Book for a fuller Account of thefe and of other Theorems.

In Chap. XIII. the Problems concerning the Lines of fwifteft Defcent, the Figures which amongft all thofe that have equal Perimeters produce

produce *Maxima* or *Minima*, and the Solid of leaſt Reſiſtance, are reſolved without Computations, from the firſt Fluxions only. There are alſo eaſy ſynthetic Demonſtrations ſubjoined, becauſe this Theory is commonly eſteemed of an abſtruſe Nature, and Miſtakes have been more frequently committed in the Proſecution of it, than of any other relating to Fluxions. To give ſome Idea of the Author's Method, ſuppoſe the Gravity to act in parallel Lines, *a* to denote the Velocity acquired at the lowermoſt Point of the Curve, and *u* the Velocity acquired at any other Point of the Curve. Suppoſe the Element of the Curve to be deſcribed by this Velocity *u*, but the Element of the Baſe to be always deſcribed by the conſtant Velocity *a*. Then it is eaſily demonſtrated without any Computation, that the Element of the Ordinate being given, the Difference of the Times in which the Elements of the Curve and Baſe are thus deſcribed is a *Minimum*, when the *Ratio* of thoſe Elements is that of *a* to *u*; *i. e.* when the Sine of the Angle, in which the Ordinate interſects the Curve, is to the *Radius* in this *Ratio*. Suppoſing therefore this Property to take Place over all the Curve, the Exceſs of the Time in which it is deſcribed by the Body deſcending along it, above the Time in which the Baſe is deſcribed uniformly with the Velocity *a*, muſt be a *Minimum*; and this latter Time being given, it follows that the Time of Deſcent in this Curve is a *Minimum*. When the Gravity tends to a given Center, ſubſtitute an Arc of a Circle deſcribed from that Center through the lowermoſt Point of the Curve in the Place of the Baſe in the former Caſe; and the Property of the Line of ſwifteſt Deſcent will be diſcovered in the ſame Manner. The Nature of the Line that among all thoſe of the ſame Perimeter is deſcribed in the leaſt Time, is diſcovered with great Facility, by determining from the former Caſe the Property of the Figure when the Sum or Difference of the Time in which it is deſcribed by the deſcending Body, and of the Time in which it would be deſcribed by any given uniform Motion, is a *Minimum*; for the latter Time being the ſame in all Curves of the ſame Length, it follows that the Figure, which has this Property, muſt be deſcribed in leſs Time than any of an equal Perimeter. The general Iſoperimetrical Problems are reſolved, and the Solutions are rendered more general, with like Facility by the ſame Method; which is alſo applied for determining the Property of the Solid of leaſt Reſiſtance, and ſerves for reſolving the Problem, when Limitations are added concerning the Capacity of the Solid, or the Surface that bounds it.

The laſt Chapter of the firſt Book treats chiefly of Gravitation towards Spheroids, of the Figure of the Planets, and of the Tides. The Author, having Occaſion in thoſe Inquiries for ſeveral new Properties of the Ellipſe, begins this Chapter by deriving it's Properties from thoſe of the Circle, by conſidering it as the oblique Section of a Cylinder, or as the Projection of the Circle by parallel Rays upon a Plane oblique to the Circle. In this Manner the Properties are briefly transferred

ferred from the one to the other, becaufe by this Projection the Center of the Circle gives the Center of the Ellipfe; Diameters perpendicular to each other in the Circle with their Ordinates, and the circumfcribed Square, give conjugate Diameters of the Ellipfe with their Ordinates, and the circumfcribed Parallelogram; parallel Lines in the Plane of this Circle are projected by Parallels in the Plane of the Ellipfe that are in the fame *Ratio*; any Area in the former is projected by an Area in the latter, which is in an invariable *Ratio* to it; and concentric Circles give fimilar concentric Ellipfes. It is likewife fhewn how Properties of a certain Kind are briefly transferred from the Circle to any conic Section with the fame Facility.

After demonftrating the Properties of the Ellipfe, it is fhewn, that if the Gravity of any Particle of a Spheroid being refolved into two Forces, one perpendicular to the Axis of the Solid, the other perpendicular to the Plane of it's Equator, then all Particles, equally diftant from the Axis, muft tend towards it with equal Forces; and all Particles at equal Diftances from the Plain of the Equator, gravitate equally towards this Plain; but that the Forces with which Particles at different Diftances from the Axis tend towards it, are as the Diftances; and that the fame is to be faid of the Forces with which they tend towards the Plain of the Equator.

From this it is demonftrated, that when the Particles of a fluid Spheroid of an uniform Denfity gravitate towards each other with Forces that are inverfely as the Squares of their Diftances, and at the fame time any other Powers act on the Particles, either in right Lines perpendicular to the Axis, that vary in the fame Proportion as the Diftances from the Axis, or in right Lines perpendicular to the Plain of the Equator, that vary as their Diftances from it, or when any Powers act on the Particles of the Spheroid, that may be refolved into Forces of this Kind; then the Fluid will be every-where *in Æquilibrio*, if the whole Force that acts at the Pole be to the whole Force that acts at the Circumference of the Equator, as the Semidiameter of the Equator to the Semiaxis of the Spheroid; and that the Forces with which equal Particles at the Surface tend towards the Spheroid, will be in the fame Proportion as Perpendiculars to it's Surface, terminated either by the Plane of the Equator, or by the Axis. Becaufe the centrifugal Force with which any Particle of the Spheroid endeavours to recede from it's Axis, in confequence of the diurnal Rotation, is as the Diftance from the Axis, it appears that if the Earth, or any other Planet, was fluid, and of an uniform Denfity, the Figure which it would affume would be accurately that of an oblate Spheroid generated by an Ellipfis revolving about it's fecond Axis.

Afterwards the Gravity towards an oblate Spheroid is accurately meafured by circular Arcs, not only at the Pole, but alfo at the Equator, and in any intermediate Places; and the Gravity towards an oblong Spheroid is meafured by Logarithms. The Gravity at any Diftance in

in the Axis of the Spheroid, or in the Plane of the Equator produced, is likewiſe accurately determined by like Meaſures, without any new Computation or Quadrature, by ſhewing that when two Spheroids have the ſame Center and *Focus*, and are of an uniform Denſity, the Gravities towards them at the ſame Point in the Axis or Plane of the Equator produced, are as the Quantities of Matter in the Solids.

This Theory is applied for determining the Figure of the Earth, by comparing the Force of Gravity in any given Latitude, derived from the Length of a *Pendulum* that vibrates there in a Second of Time, with the centrifugal Force at the Equator, deduced from the periodic Time of the diurnal Rotation, and the Amplitude of a Degree of the Meridian; or by comparing the Lengths of *Pendulums* that vibrate in equal Times in given unequal Latitudes; or by comparing different Degrees meaſured upon the Meridian. By the beſt Obſervations it would ſeem, that there is a greater Increaſe of Gravitation, and of the Degrees of the Meridian from the Equator towards the Poles, than ought to ariſe from the Suppoſition of an uniform Denſity. Therefore the Author ſuppoſes the Denſity to vary from the Surface towards the Center; and, in ſeveral Caſes he has conſidered, he finds that a greater Denſity towards the Center would account for a greater Increaſe of Gravitation, towards the Poles, but not for a greater Increaſe of the Degrees of the Meridian; and that the Hypotheſis of a leſs Denſity towards the Center would account for the latter, but not for the former, ſuppoſing (after Sir *I. Newton*) the Columns of the Fluid to extend from the Surface to the Center, and there to ſuſtain each other. On this Account he determines the Gravitation towards the Earth, when it is ſuppoſed to be hollow with a *Nucleus* included, according to the Hypotheſis advanced by Dr *Halley*, with the Difference of the Semidiameters that might ariſe from ſuch a Diſpoſition of the Internal Parts. But in this Caſe, and when the Denſity is ſuppoſed variable, the ſpheroidical Figure is only aſſumed as an Hypotheſis. He adds, that by imagining the Denſity to be greater in the Axis than in the Plain of the Equator at equal Diſtances from the Centre, an Hypotheſis perhaps might be found, that would account for moſt of the *Phænomena*; but that a Series of many exact Obſervations is requiſite, before we can examine with any Certainty the various Suppoſitions that may be imagined concerning the internal Conſtitution of the Earth. This Doctrine is likewiſe applied for determining the Figure of *Jupiter*.

It follows from the ſame Theorem, that if we ſuppoſe the Earth to be fluid, and abſtract from it's Motion upon it's Axis, and the Inclination of the right Lines in which it's Particles gravitate towards the Sun or Moon, the Figure which it would aſſume in conſequence of the unequal Gravitation of it's Particles towards either of thoſe Bodies would be accurately that of an oblong Spheroid having it's Axis directed towards that Body. The Aſcent of the Water, deduced from this Theorem, agrees nearly with that which Sir *I. Newton* found, by computing

puting it briefly from what he had demonſtrated concerning the Figure of the Earth. Several Obſervations are ſubjoined concerning the Tides, and the Cauſes which may contribute to increaſe or diminiſh them, particularly the Inequality of the Velocities with which Bodies revolve about the Axis of the Earth in different Latitudes.

This Chapter concludes by demonſtrating briefly, that if the Attraction of the Particles decreaſed as the Cube of their Diſtance increaſes, or in any higher Proportion, then any Particle would tend toward the leaſt Portion of Matter in Contact with it, with a greater Force than towards the greateſt Body at any Diſtance, how ſmall ſoever from it. The true Law of Gravity is better adapted for holding the Parts of each Body in a proper Union, while it perpetuates the Motions in the great Syſtem about the Sun, and preſerves the Revolutions in the leſſer Syſtems nearly regular; and the Author concludes with obſerving, that a remarkable geometrical Simplicity is often found in the Concluſions that are derived from it.

V. 3. In the ſecond Book, he treats of the Method of Computation, or the Algebraic Part; to the Facility, Conciſeneſs, and great Extent of which, the Improvements that have been made by this Method are in great meaſure to be aſcribed. In order to obtain thoſe Advantages, it was neceſſary to admit various Symbols into the Algebra: But the Number and Complication of thoſe Signs muſt occaſion ſome Obſcurity in this Art, unleſs Care be taken to define their Uſe and Import clearly, with the Nature of the ſeveral Operations. An Example of this is given by an Illuſtration of one of the firſt Rules in Algebra. As it is the Nature of Quantity to be capable of Augmentation and Diminution, ſo Addition and Subſtraction are the primary Operations in the Sciences that treat of it. The poſitive Sign implies an Increment, or a Quantity to be added. The negative Sign implies a Decrement, or Quantity to be ſubſtracted: And theſe ſerve to keep in our View what Elements enter into the Compoſition of Quantities, and in which Manner, whether as Increments or Decrements. It is the ſame Thing to ſubſtract a Decrement as to add an equal Increment. As the Multiplication of a Quantity by a poſitive Number implies a repeated Addition of the Quantity, ſo the Multiplication by a negative Number implies a repeated Subſtraction: And hence to multiply a negative Quantity, or Decrement, by a negative Number, is to ſubſtract the Decrement as often as there are Units in this Number, and therefore is equivalent to adding the equal Increment the ſame Number of Times; or, when a negative Quantity is multiplied by a negative Number, the Product is poſitive. When we inquire into the Proportion of Lines in Geometry, we have no Regard to their Poſition or Form; and there is no Ground for imagining any other Proportion betwixt a poſitive and negative Quantity in Algebra, or betwixt an Increment and a Decrement, than that of the abſolute Quantities or Numbers themſelves. The Algebraic Expreſſions, however, are chiefly uſeful, as they

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ferve to reprefent the Effects of the Operations; and fuch Expreffions are not to be fuppofed equal that involve equal Quantities, unlefs the Operations denoted by the Signs are the fame, or have the fame Effect. Nor is every Expreffion to be fuppofed to reprefent a certain Quantity; for if the $\sqrt{-1}$ fhould be faid to reprefent a certain Quantity, it muft be allowed to be imaginary, and yet to have a real Square; a way of fpeaking which it is better to avoid. It denotes only, that an Operation is fuppofed to be performed on the Quantity that is under the radical Sign. The Operation is indeed in this Cafe imaginary, or cannot fucceed; but the Quantity that is under the radical Sign, is not lefs real on that Account. The Author mentions thofe Things briefly, becaufe they belong rather to a Treatife of Algebra than of Fluxions, wherein the common Algebra is admitted.

In order to avoid the frequent Repetition of figurative Expreffions in the Algebraic Part, the Fluxions of Quantities are here defined to be any Meafures of their refpective Rates of Increafe or Decreafe, while they are fuppofed to vary (or flow) together. Thefe may be determined by comparing the Velocities of Points that always defcribe Lines proportional to the Quantities, as in the firft Book; but they may be likewife determined, without having Recourfe to fuch Suppofitions, by a juft Reafoning from the fimultaneous Increments or Decrements themfelves. While the Quantity A increafes by Differences equal to a, $2\,A$ increafes by Differences equal to $2\,a$, and (fuppofing m and n to be invariable) $\frac{m\,A}{n}$ increafes by Differences equal to $\frac{m\,a}{n}$ and therefore at a greater or lefs Rate than a, in Proportion as m is greater or lefs than n. Thus a Quantity may be always affigned that fhall increafe at a greater or lefs Rate than A, (*i. e.* fhall have it's Fluxion greater or lefs than the Fluxion of A) in any Proportion; and a Scale of Fluxions may be eafily conceived, by which the Fluxions of any other Quantities of the fame Kind may be meafured.

Let B be any other Quantity whofe Relation to A can be expreffed by any Algebraic Form; and while A increafes by equal fucceffive Differences, fuppofe B to increafe by Differences that are always varying. In this Cafe, B cannot be fuppofed to increafe at any one conftant Rate; but it is evident, that if B increafe by Differences that are always greater than the equal fucceffive Differences by which $\frac{m\,A}{n}$ increafes at the fame Time, then B cannot be faid to increafe at a lefs Rate than $\frac{m\,A}{n}$; or if the Fluxion of A be reprefented by a, the Fluxion of B cannot be lefs than $\frac{m\,a}{n}$. And if the fucceffive Diffe-

rences

rences of B be always lefs than thofe of $\frac{m\,A}{n}$, then furely B cannot be faid to increafe at a greater Rate than $\frac{m\,A}{n}$; or the Fluxion of B cannot be faid to be greater in this Cafe than $\frac{m\,a}{n}$.

From thofe Principles the primary Propofitions in the Method of Fluxions, and the Rules of the direct Method, with the Fundamental Rules of the inverfe Method, are demonftrated. We muft be brief in our Account of the Remainder of this Book. The Rule for finding the Fluxion of a Power is not deduced, as ufually, from the Binomial Theorem, but from one that admits of a much eafier Demonftration from the firft Algebraic Elements, *viz.* That when n is any integer pofitive Number, if the Terms E^{n-1}, $E^{n-2}F$, $E^{n-3}F^{2}$, $E^{n-4}F^{3}$, ... F^{n-1}, (wherein the Index of E conftantly decreafes, and that of F increafes by the fame Difference Unit) be multiplied by $E-F$, the Sum of the Products is $E^{n}-F^{n}$; from which it is obvious, that when E is greater than F, then $E^{n}-F^{n}$ is lefs than $nE^{n-1}\times\overline{E-F}$ but greater than $nF^{n-1}\times\overline{E-F}$.

The Rules are fometimes propofed in a Form fomewhat different from the ufual Manner of defcribing them, with a View to facilitate the Computations both in the direct and inverfe Method. Thus, when a Fraction is propofed, and the Numerator and Denominator are refolved into any Factors, it is demonftrated, that the Fluxion of the Fraction divided by the Fraction is equal to the Sum of the Quotients, when the Fluxion of each Factor of the Numerator is divided by the Factor itfelf, diminifhed by the Quotients that arife by dividing in like Manner the Fluxion of each Factor of the Denominator by the Factor.

The Notation of Fluxions is defcribed in Chap. 2. with the Rules of the direct Method, and the fundamental Rules of the inverfe Method. The latter are comprehended in Seven Propofitions, Six of which relate to Fluents that are affignable in finite Algebraic Terms, and the Seventh to fuch as are affigned by infinite Series. It is in this Place the the Author treats of the Binomial and Multinomial Theorems (becaufe of their Ufe on this Occafion), and they are inveftigated by the direct Method of Fluxions. The fame Method is applied for demonftrating other Theorems, by which an Ordinate of a Figure being given, and it's Fluxions determined, any other Ordinate and *Area* of the Figure may be computed. The moft ufeful Examples are defcribed in this Chapter, by computing the Series's that ferve for determining the Arc from it's Sine or Tangent, and the Logarithm from it's Number, and converfely the Sine, Tangent, or Secant, from the Arc, and the Number from it's Logarithm.

The

The inverſe Method is proſecuted farther in Chapter III. by reducing Fluents to others of a more ſimple Form, when they are not aſſignable by a finite Number of Alegebraic Terms. When a Fluent can be aſſigned by the Quadrature of the Conic Sections, (and conſequently by circular Arcs or Logarithms) this is conſidered as the ſecond Degree of Reſolution; and this Subject is treated at Length. An Illuſtration is premiſed of the Analogy betwixt Elliptic and Hyperbolic Sectors formed by Rays drawn from the Centers of the Figures: The Properties of the latter are ſometimes more eaſily diſcovered becauſe of their Relation to Logarithms, and lead us in a brief Manner to the analogous Properties of elliptic Sectors, and particularly to ſome general Theorems concerning the Multiplication and Diviſion of circular Sectors or Arcs. When two Points are aſſumed in an Hyperbola, and alſo in an Ellipſis, ſo that the Sectors terminated by the Semi-axis; and the two Semi-diameters, belonging to thoſe Points, are in the ſame given *Ratio* in both Figures, then the Relation betwixt the Semi-axis and the two Ordinates drawn from thoſe Points to the other Axis, is always defined by the ſame, or by a ſimilar Equation in both Figures. This Propoſition ſerves for demonſtrating Mr *Cotes*'s celebrated Theorem, as it is extended by M. *De Moivre*, by which a Binomial or Trinomial is reſolved into it's quadratic Diviſors, and various Fluents are reduced to circular Arcs and Logarithms. The Demonſtrations are alſo rendred more eaſy of the Theorems concerning the Reſolution of a Fraction, that has a multinomial Denominator, into Fractions that have the ſimple or quadratic Diviſors of the Multinomial for their ſeveral Denominators. Theſe Demonſtrations are derived from the Method of Fluxions itſelf, without any foreign Aid; the invariable Coefficients being determined by ſuppoſing the variable Quantity or it's Fluxions to vaniſh.

When a Fluent cannot be aſſigned by the Areas of Conic Sections, it may however be meaſured by their Arcs in ſome Caſes; and this may be conſidered as the third Degree of Reſolution, or the Fluents may be called of the third Order. On this Occaſion, ſome Fluents are found to depend on the Rectification of the Hyperbola and Ellipſis, which have been formerly eſteemed of an higher Kind. The Conſtruction of the elaſtic Curve, with it's Rectification, and the Meaſure of the Time of Deſcent in an Arch of a Circle, are derived from hyperbolic and elliptic Arcs; and the Fluents of this Kind are compared with thoſe of the firſt or ſecond Order by infinite Series. Becauſe there are Fluents of higher Kinds than theſe, the Trajectories abovementioned, which are deſcribed by a centripetal Force, that is, as ſome Power of the Diſtance from a given Centre, when the Velocity of the Projection is that which would be acquired by an infinite Deſcent, or by ſuch a centrifugal Force, and the Velocity is ſuch as would be acquired by flying from the Centre, are employed for repreſenting them. A ſimple Conſtruction of theſe Trajectories had been given above, by drawing Rays

Rays from the Centre to a Right Line given in Pofition, increafing or diminifhing the Logarithms of thofe Rays always in a given *Ratio*, and increafing or diminifhing the Angles contained by them and the Perpendicular in the fame *Ratio*. From any Figure of this Kind, a Series of Figures is derived by determining the Interfections of the Tangents of the Figure with the Perpendiculars from the Centre. Every Series of this Kind gives two diftinct Sort of Fluents; and any one Fluent being given, all the other Fluents taken alternately from it in the Series depend upon it, or are meafured by it; but it does not appear, that the Fluents of one Sort can be compared with thofe of the other Sort, or with thofe of any different Series of this Kind.

The inverfe Method is profecuted farther in the 4th Chapter, by various Theorems concerning the Area when the Ordinate is expreffed by a Fluent, or when the Ordinate and Bafe are both expreffed by Fluents. The firft is the XIth Prop. of Sir *I. Newton*'s Treatife of Quadratures. In Art. 819, 820, *&c.* the Author fuppofes the Ordinate and Bafe to be both expreffed by Fluents, and fhews, in many Cafes, that the Area may be affigned by the Product of two fimple Fluents, as of two circular Arcs, or of a circular Arc and a Logarithm. This Subject deferves to be profecuted, becaufe the Refolution of Problems is rendered more accurate and fimple, by reducing Fluents to the Products of Fluents already known, than by having immediately Recourfe to infinite Series. One of the Examples in Art. 822, may be eafily applied for demonftrating, that the Sum of the Fractions which have Unit for their common Numerator, and the Squares of the Numbers 1, 2, 3, 4, 5, 6, *&c.* in their natural Order, for their fucceffive Denominators, is one fixth Part of the Number, which expreffes the *Ratio* of the Square of the Periphery of a Circle to the Square of it's Diameter; which is deduced by Mr *Euler*, *Comment. Petropol.* Tom. 7. in a different Manner; and other Theorems of this Kind may be demonftrated from the fame or like Principles.

The Series that is deduced by the ufual Methods for computing the Area or Fluent, converge in fome Cafes at fo flow a Rate, as to be of little or no Ufe without fome farther Artifice. For Example: The Sum of the firft Thoufand Terms of Lord *Brounker*'s Series for the Logarithm of 2, is deficient in the fifth Decimal. In order therefore to render the Account of the inverfe Method more complete, the Author fhews how this may be remedied in many Cafes, by Theorems derived from the Method of Fluxions itfelf, which likewife ferve for approximating readily to the Values of Progreffions, and for refolving Problems that are commonly referred to other Methods. Thofe Theorems had been defcribed in Book I. Art. 352, *&c.* but the Demonftration and Examples were referred to this Place, as requiring a good deal of Computation. The Bafe being fuppofed equal to Unit, and it's Fluxion alfo equal to Unit, let half the Sum of the extreme Ordinates be reprefented by a, the Difference of the firft Fluxions of thefe Ordi-

nates by b, the Difference of their third, fifth, ſeventh, and higher alternate Fluxions by c, d, e, &c. then the Area ſhall be equal to $a - \frac{b}{12} + \frac{c}{720} - \frac{d}{30240} + \frac{e}{1209600} -$, &c. which is the firſt Theorem for finding the Area. The reſt remaining, let a now repreſent the middle Ordinate, and the Area ſhall be equal $a + \frac{b}{24} - \frac{7c}{5760} + \frac{31d}{967680} - \frac{127e}{154828800} +$, &c. And this is the Theorem which the Author makes moſt Uſe of. When the ſeveral intermediate Ordinates repreſent the Terms of a Progreſſion, the Area is computed from their Sum, or converſely their Sum is derived from the Area, by Theorems that eaſily flow from theſe.

Theſe general Theorems are afterwards applied for finding the Sums of the Powers of any Terms in Arithmetical Progreſſion, whether the Exponents of the Powers be poſitive or negative, and for finding the Sums of their Logarithm, and thereby determining the *Ratio* of the *Uncia* of the middle Term of a Binomial of a very high Power to the Sum of all the *Unciæ*. This laſt Problem was celebrated amongſt Mathematicians ſome Years ago, and by endeavouring to reſolve it by the Method of Fluxions the Author found thoſe Theorems, which give the ſame Concluſions that are derived from other Methods. They are likewiſe applied for computing *Areas* nearly from a few equidiſtant Ordinates, and for interpolating the intermediate Terms of a Series, when the Nature of the Figure can be determined, whoſe Ordinates are as the Differences of the Terms.

In the laſt Chapter, the general Rules derived from the Method of Fluxions for the Reſolution of Problems, are deſcribed and illuſtrated by Examples. After the common Theorems concerning Tangents, the Rules for determining the greateſt and leaſt Ordinates, with the Points of contrary Flexure, and the Precautions that are neceſſary to render them accurate and general, (which were deſcribed above) are again demonſtrated. Next follow the Algebraic Rules for finding the Center of Curvature, and determining the Cauſtics by Reflexion and Refraction, and the centripetal Forces. The Conſtruction of the Trajectory is given, which is deſcribed by a Force that is inverſely as the 5th Power of the Diſtance from the Center, becauſe this Conſtruction requires Hyperbolic and Elliptic Arcs, and becauſe a remarkable Circumſtance takes Place in this Caſe, (and indeed in an Infinity of other Caſes) which could not obtain in thoſe that have been already conſtructed by others, *viz.* That a Body may continually deſcend in a ſpiral Line towards the Center, and yet never approach ſo near to it as to deſcend to a Circle of a certain *Radius*; and a Body may recede for ever from the Center, and yet never ariſe to a certain finite Altitude. The Conſtruction of the Caſes wherein this obtains is performed

formed by Logarithms or Hyberbolic Areas, the Angles defcribed about the Center being always proportional to the Hyperbolic Sectors, while the Diftances from the Center are directly or inverfely as the Tangents of the Hyperbola at it's Vertex. The Circle is an Afymptote to the Spiral; and this can never be, unlefs the Velocities requifite to carry Bodies in Circles increafe while the Diftances decreafe, (or decreafe while the Diftances increafe) in a higher Proportion than the Velocity in the Trajectory; that is, unlefs the Force be inverfely as a higher Power of the Diftance than the Cube. Next follow Theorems for computing the Time of Defcent in any Arc of a Curve, for finding the Refiftance and Denfity of the Medium when the Trajectory and centripetal Force are given, and for defining the *Catenaria* and Line of fwifteft Defcent in any Hypothefis of Gravity.

Then the ufual Rules are derived from the inverfe Method for computing the Area, the Solid generated by it, the Arc of the Curve, and the Surface defcribed by it revolving about a given Axis. The meridional Parts in a Sphere, and any Spheroid, are determined with the fame Accuracy, and almoft equal Facility. The Attraction of a Spheroid at the Equator, as well as at the Poles, is determined in a more general Manner than in the firft Book, or in a Piece of the Author's publifhed at *Paris* in 1740, which obtained a Part of the Prize propofed by the *Royal Academy of Sciences* for that Year. Several Mechanical Problems are refolved, concerning the Proportion the Power ought to bear to the Weight, that the Engine may produce the greateft Effect in a given Time; and concerning the moft advantageous Pofition of a Plane which moves parallel to itfelf, that a Stream of Air or Water may impel it with the greateft Force, having Regard to the Velocity which the Plane may have already acquired. On this Occafion, it is fhewn, that the Wind ought to ftrike the Sails of a Wind-mill in a greater Angle than that of $54^\circ\ 44'$, againft what has been deduced from the fame Principles by a learned Author. The fame Theory is applied to the Motion of Ships, abftracting from the Lee-way, but having Regard to the Velocity of the Ship; and amongft other Conclufions it appears, that the Velocity of a Veffel of one Sail may be greater with a Side-wind, than when fhe fails directly before the Wind; which, perhaps, may be the Cafe of thofe feen by Captain *Dampier* in the *Ladrone Iflands*, that failed at the Rate of 12 Miles in half an Hour with a Side-wind.

The Remainder of this Chapter is employed in reducing Equations from fecond to firft Fluxions; conftructing the elaftic Curve by the Rectification of the equilateral Hyperbola; determining the Vibrations of Mufical Chords; refolving Problems concerning the *Maxima* and *Minima*, that are propofed with Limitations, relating to the Perimeter of the Figure, it's Area, the Solid generated by this Area, &c. with Examples of this Kind concerning the Solid of leaft Refiftance; and concludes with an Inftance of the Theorems by which the Value of the Ordi-

Ordinate may be determined from the Value of the Area, by common Algebra, and by obferving, that it is not abfolute, but relative Space and Motion, that is fuppofed in the Method of Fluxions.

A general Method of defcribing Curves, by the Interfection of right Lines moving about Points in a given Plane, by the Rev. Mr William Braikenridge, No. 436. p. 25. *Jan.* &c. 1735.

VI. 1. You have here a general Method of defcribing Lines of any Order, by means of the Interfection of right Lines about Poles; which is much more fimple than that of Sir *I. Newton*, and will give a Solution of many very difficult Problems; and I queftion whether they can be found by any other Principles. I gave only one particular Cafe of this in a geometrical Exercitation printed at *London* in 1733, not thinking it convenient to explain the whole Affair at that Time, tho' I was well acquainted with the Method. It is now three Years ago, that I fell upon the general Theorem, but I had many Reafons for concealing it; and I was determined to let two Years at leaft pafs after the Publication of that Exercitation, before this general Method fhould come into the World. For I did not doubt, but that if any others were in Poffeffion of this Invention, they would, upon the Publication of a particular Cafe, efpecially as they were provoked to it, lay hold of the Opportunity to publifh their general Method, if they had really difcovered any.

Fig. 6. About three given Points A, B, C, as Poles, in any Plane, let there be moved three right Lines, A N S, B O S, C N O, which may interfect one another in the Points S, N, O, and let the two Points of interfection S and N be drawn through the right Lines D K S, R N K given by Pofition; the reft O will defcribe a Conic Section. If through the Points, A, B, C, are drawn the right Lines A B, A C, meeting each other in A, and the right Lines R K, D K, given by Pofition in R and M; the Figure defcribed will pafs through the five Points B, C, K, M, R. And hence appears a new Method of defcribing a Conic Section through five given Points, much more eafy than any that have been hitherto invented.

Demonftrated in Exerc. Geom. *Prop.* 1.

Vid. Exerc. Geom. *Prop.* 3.

Fig. 7. Let there be moved about four Points A, B, C, D, as Poles, in any Plane, as many right Lines, A N S, B O S, C N O, D P O, three of which A N S, B O S, C N O, may interfect each other in three Points, S, N, O, and let the two Points of Interfection S, N, be drawn through the right Lines d K, R K, given by Pofition, and in the mean time let the right Line D P O, drawn from the fourth Pole O, pafs through the Remainder O, and cut the right Line A N S in P, and that Point P will defcribe a Line of the third Order.

Demonftrated in Exerc. Geom. *Prop.* 11.

Through the Poles A, B, D, let the right Lines A B R, D B H, be drawn, meeting each other in B, and the right Lines K R, K d, given by Pofition in R H; the Figure defcribed by the Motion of the Point P will pafs through the five Points, A, D, H, K, R, of which A will be double. Hence is deduced the Method of defcribing a Line of the 3d Order through feven given Points, one of which is double. For

Fig. 8. let A, D, H, K, P, M, R, be given, and one of them A muft be double. Through the two Points H, R, and another K, let the right Lines

Lines H K, R K, be moved, and let the Points A R, and H D, be joined, and let the right Lines A R, H D, be produced, meeting one another in B. Then the right Lines A P N S, A M n s, cutting the right Line K R in N, n, and the right Line H K, in S, s, being drawn through A, and the Points P, M; let the right Lines B S, B s, be drawn through thofe Points S, s, to B; and through D, to the Points P, M, move the right Lines D P O, D M T, meeting the right Lines B S, B s, in O, T. Let the Points O, N, and T, n be joined, and let the right Lines O N, T n be produced, meeting together in C. Then about the Points A, B, C, D, as Poles, let the right Lines A S, B O, C O, DO, revolve, of which let three A S, B O, C O, interfect each other in the Points S N O, and let two S, N be drawn through the right Lines H K, K R, and in the mean time let the right Line D O always pafs through the Remainder O, and cut the right Line A N S in P, and this Interfection P of the right Lines A S, D O, will defcribe a Line of the third Order paffing through feven given Points, A, D, H, K, M, P, R, and doubly through the given A.

Lines alfo of the third Order are more generally, but lefs commodioufly defcribed after this Manner, which alfo comprehends the firft. About five given Points A, B, C, D, E, as Poles, let as many right Lines A N S, B O S, C N O, D P O E P S revolve, of which let three A N S, B O S, C N O interfect one another in the Points N S O; let two S, N, be drawn through the right Lines given by Pofition d K, K R; and one S of the two S, N, and the Remainder O, let the right Lines E P S, D P O pafs, being drawn through the Poles E, D, and meeting in P: let that Point P defcribe a Line of the third Order, with a double Point in the Pole E. Fig. 9.

In like Manner may Lines of the fourth order be defcribed. About five given Points joined A, B, C, D, E, as Poles, in any Plane; let as many right Lines, A N S, B Q S, C N O, D P O, E P Q, be moved; of which let three A N S, B Q S, C N O, meet each other in three Points S, N, O; let the two Points of Interfection be drawn thro' the right Lines d K, R K, given by Pofition, and in the mean time let the right Line D P O, moveable about the fourth Pole D, pafs thro' the Remainder O, and cut the right Line A N S, in P; then let the right Line E P Q, drawn from the 5th Pole E, be drawn thro' P, and be produced on both Sides, till it meets the right Lines B Q S, C N O, in Q and W: I fay that the Points Q, W, will defcribe Lines of the 4th Order. Through the Poles A, E, and B, D, let the right Lines A E H, B D F revolve, meeting the right Line d K, given byPofition, in H, F, let D and E be joined; and A D being drawn through the Poles D, A, meeting the right Line d K in V; from V let the right Line V B be drawn to the Pole B, and cut the right Line D E in G. The Figure defcribed will pafs through the five Points B, E, G, F, H, and triply through the Pole B. Let the right Line A, B, R, be produced through the Poles A, B, and meet the right Fig. 10. *Demonftrated from* Exerc. Geom. *Prop.* 11.

right Line K R, given by Position in R; the Curve will also pass thro' the Points R, K.

Fig. 11. Hence is derived the Method of drawing a Line of the 4th Order through nine given Points, of which one is triple. For let B, E, F, G, H, L, M, T, Q be given, and one of them B must be triple. Let the Points B F, F H, H E be joined, and the right Lines B F, F H, H E be produced, and through the Points E G, G B, let the right Lines E G D, B G V be drawn, of which let E G D cut the right Line B F in D, and let the other B G V cut the right Line F H in V. Then having joined V and D, and produced V D, till it meets the right Line H E in A, let the right Line d A B R be drawn thro' the Points A, B. Then from the Points B, E, let the right Lines B Q S, E P Q, be moved to the given Q, of which let the first B Q S meet with F H, produced to S; and A S being drawn through the Points A, S, and meeting the right Line E Q in P, let the right Line D P O be produced through P and D, and meet the right Line B Q S in O: and let the Point O be marked. And in like Manner from the same B, E, to another given T, let the right Lines B T s, E p T (supply the Figure) be turned, of which let B T s meet with F H in s, and the right Line A s cutting the right Line E p T in p being drawn, let the right Line D p Z be moved through p and D, and meet the right Line B T s in Z, and let Z be marked. And so on let right Lines be drawn from the same B, E, to the other given M L, and right Lines being drawn from A and D as before, let the Points found be marked X Y. Then thro' the four Points found, O, Z, X, Y, and the given one B, let a Conic Section be described, cutting the right Line F H in the Points I K, and the right Line d A B in B, R. Through the Points A, I, let the right Line A I be drawn, cutting the Conic Section in I and C; and let the Points K, R be joined, and let the right Line K R be produced. Now about the five Points A, B, C, D, E, as Poles, let as many right Lines A S, B S, C N, D O, E Q, revolve, of which let three A S, B S, C N, meet each other in N, S, O, and let the Intersections N and S of the right Lines A S, C N, and A S, B S be drawn through the right Lines K R, F H K; and in the mean time let the right Line D P O pass through the Pole D, and the Intersection O of the right Lines B S, C N, and cut the right Line A S in P; and through P and the Pole E, let the right Line E P Q be produced, cutting the right Line B S, in Q, and this Intersection Q of the right Lines B S, E P will describe a Line of the 4th Order passing through nine given Points, B, E, F, G, H, L, M, T, Q, one of which B will be triple.

Vid. Exerc. Geom. *Prop.* 3.

By a Method not much unlike this, a Line of the 4th Order may be described through eight given Points, three of which are double, as also a Line of the same Order through eleven given Points, two of which are double, and more of the same Sort.

2 But

Fig. 6.

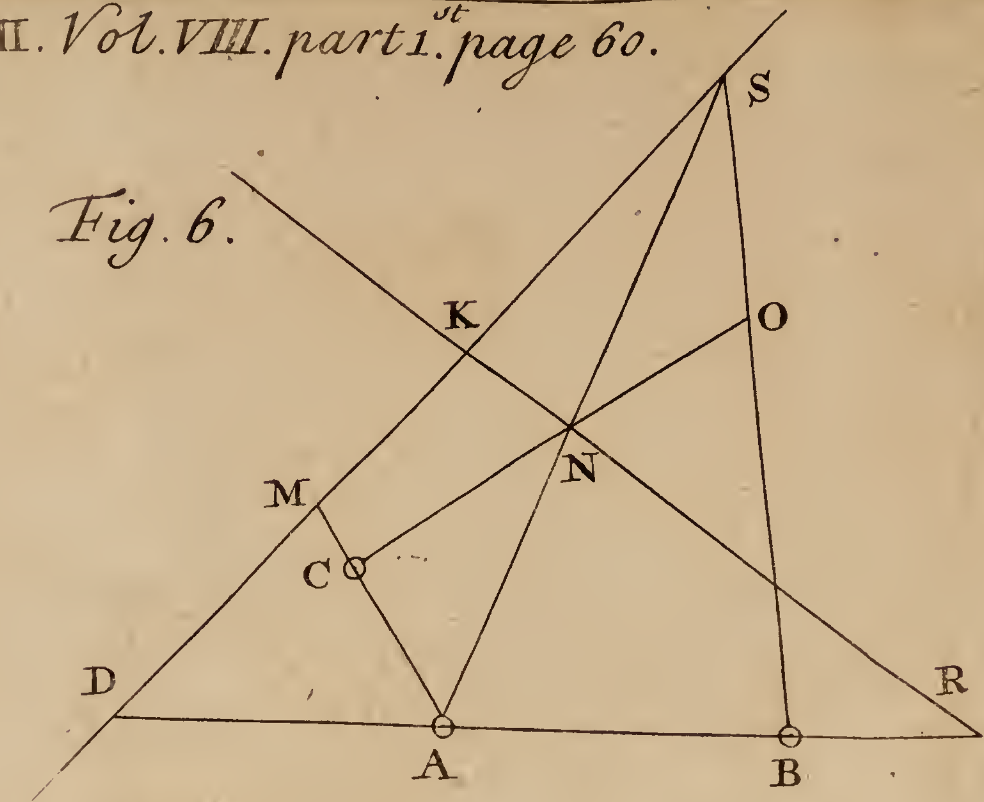


Fig. 7.

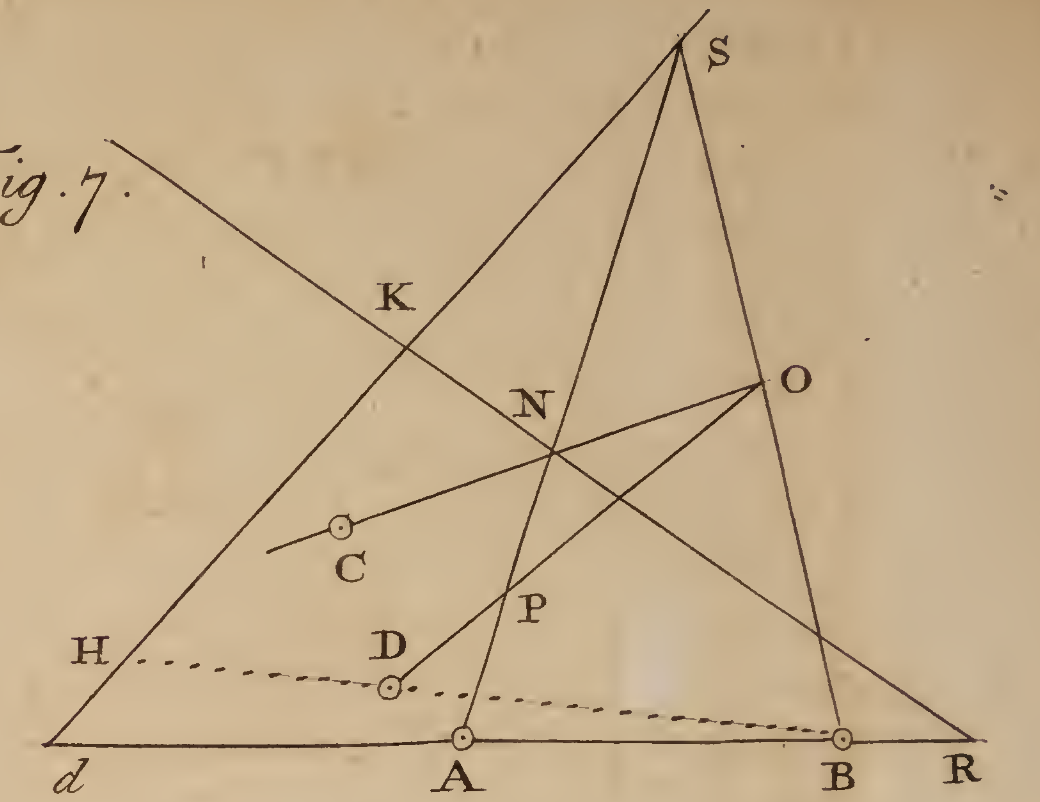


Fig. 8.

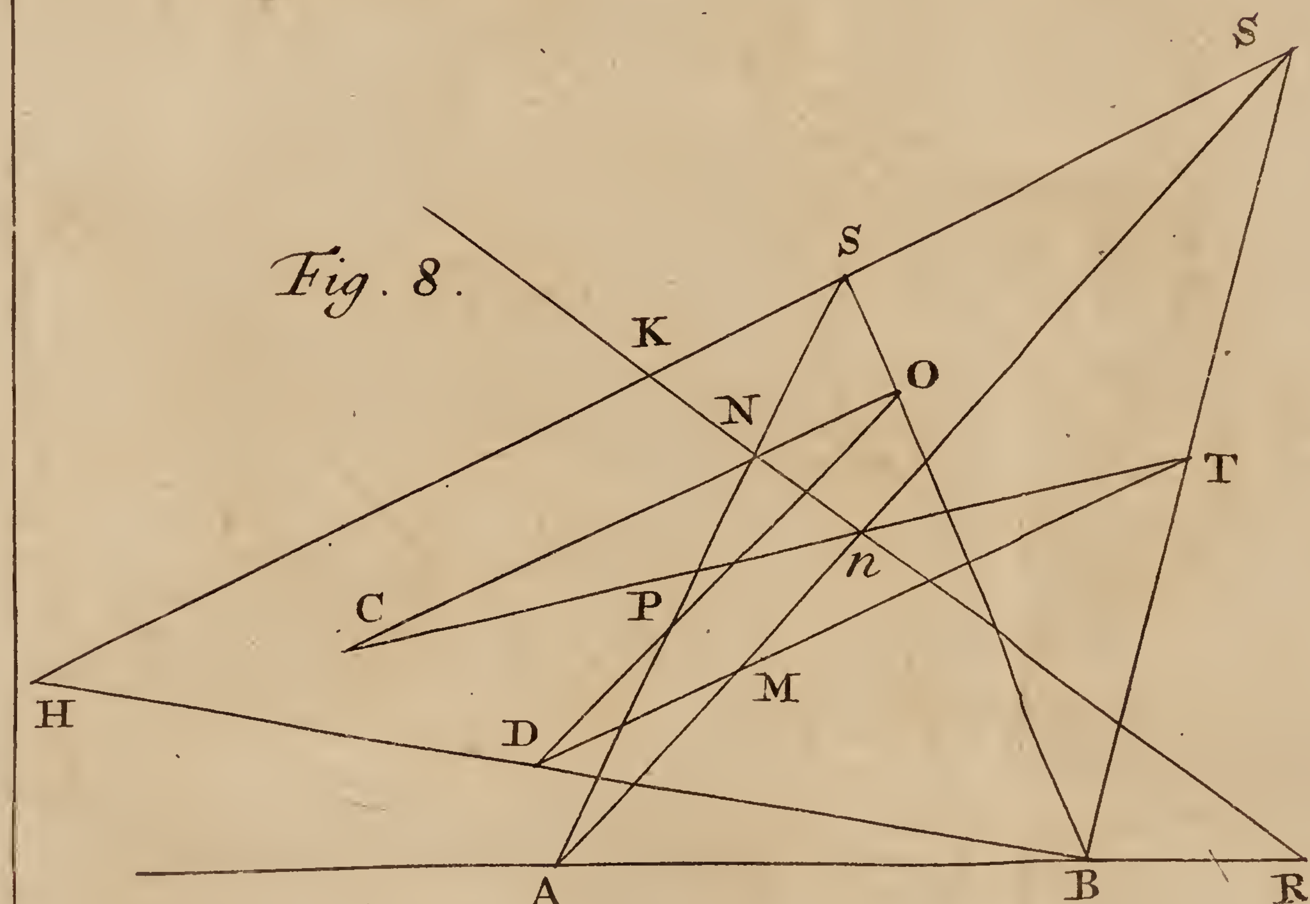


Fig. 9.

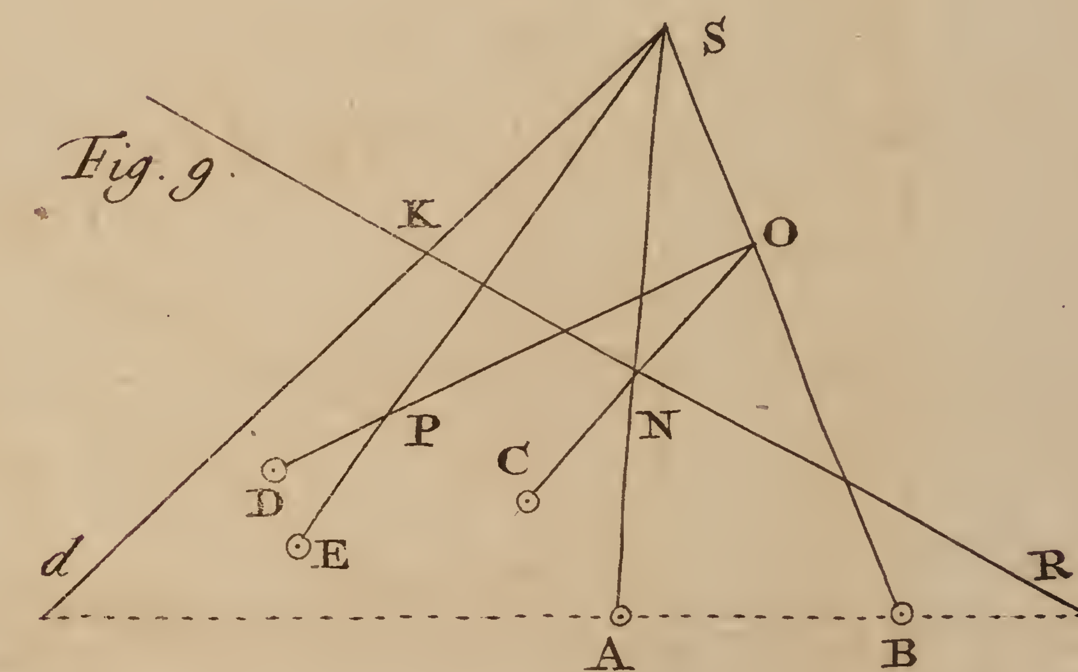


Fig. 10.

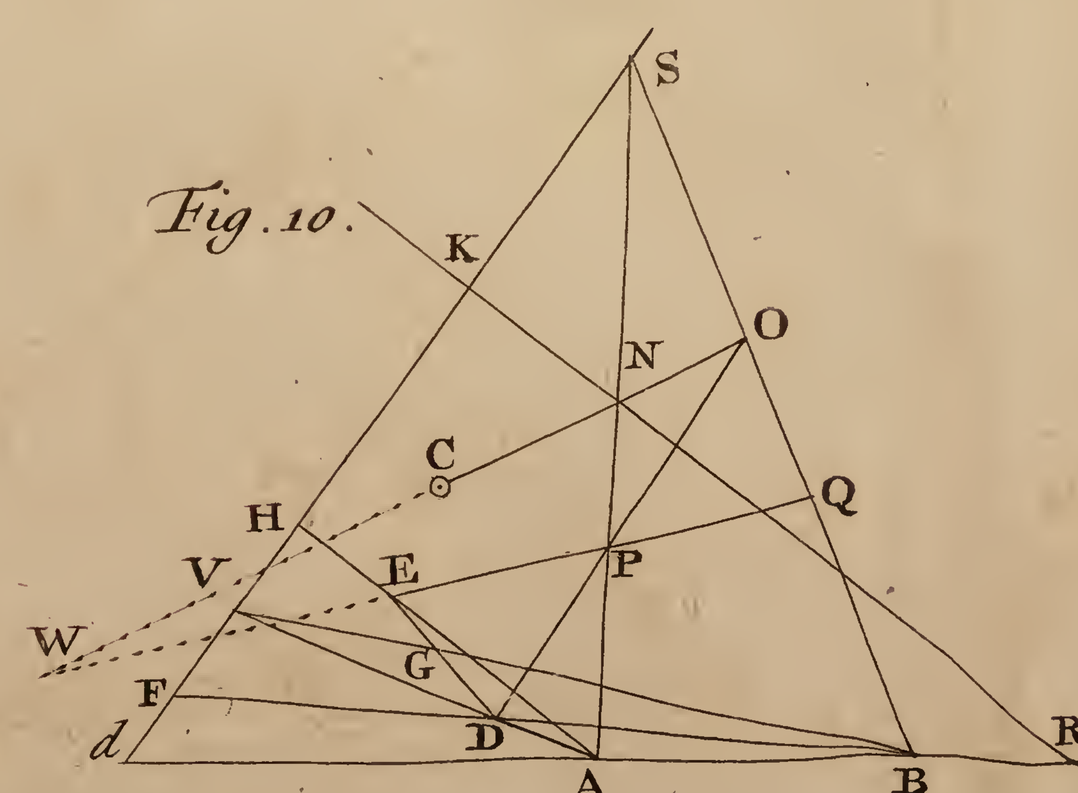
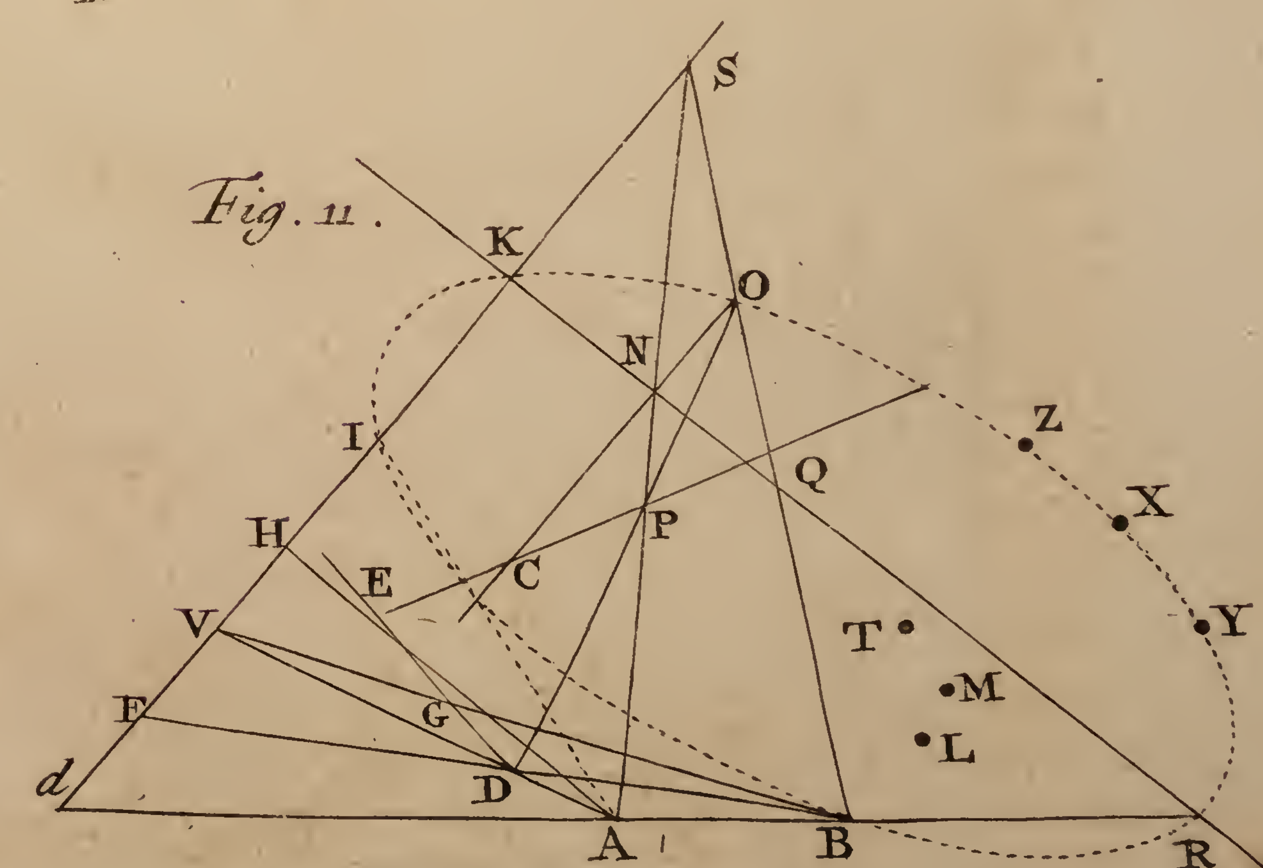


Fig. 11.



But as for the Number of Points which determine a Line of any Order, I find that, if n is the Number of the Dimenſions of a Line, $n^2 + 1$ will be the Number of Points through which the Line may be deſcribed. For Inſtance, a Line of the ſecond Order through five Points; of the third through 10; of the 4th through 17; of the 5th through 26. And hence is deduced, that if a Line of the Order n has a *punctum multiplex*, it may be deſcribed through $2n + 1$. For Example, a Line of the third Order, with a *punctum duplex*; that is, $n - 1 = 2$, thro' ſeven Points, and a Line of the 4th Order with a *punctum triplex* through nine, *&c.* And generally if p, q, r, &c. denote *puncta multiplicia*, of which the Number is m, a Curve may be deſcribed through $n^2 - p^2 - q^2 - r^2 + m + 1$ Points, in which m are *multiplicia*; for Inſtance, a Line of the 4th Order, which has three *puncta duplicia* may be deſcribed through eight Points; for $n = 4$, $p = q = r = 2$, $m = 3$, and $16 - 4 - 4 - 4 + 3 + 1 = 8$.

There is another Method alſo, not very different from the firſt, of deſcribing Lines of the 4th Order, but a little more complicated. About ſeven Poles A, B, C, D, E, F, G, let there revolve as many right Lines A S, B S, C N, D S, E N, F O, G T, of which let one A N S, in revolving cut the right Lines d K, R K, given by Poſition, in the Points S, N; let the right Lines C N, E N be drawn through one of them N, and the right Lines B S, D S through the other S, and meet the right Lines C N, E N in the Points O, T, deſcribing Conic Sections as above; and in the mean time let the right Lines F O, G T, drawn from the Poles F, G, paſs through the ſame O, T, and meet in P; the interſection P will deſcribe a Line of the 4th Order, with a double Point in both Poles F and G. Fig. 12.

But not to detain you any longer with theſe, I ſhall now give you the general Theorem. About the Points A, B, C, D, E, F, G, H, *&c.* as Poles, of which let the Number be n, let as many right Lines A S, B S, C N, D P, E Q, F W, X G, H Y, *&c.* revolve, of which let three A S, B S, C N interſect each other in the Points N, S, O, let two, S, N, be drawn through the right Lines d K, K R given by Poſition; and in the mean while through the Remainder O and the Pole D let the right Line D P paſs, cutting the right Line A S in P, and the right Line E, Q, being drawn through P and the Pole E, cutting the right Line B S in Q, and from Q through the Pole F, let F Q be drawn, and cut the right Line A S in W, and W G being drawn through W and the Pole G, cutting the right Line B S in X, and then let the right Line H Y be produced through X and the Pole H, meeting the right Line S A in Y, and ſo on; the Interſection Y of the right Line Y H drawn from the laſt Pole H, with either of the right Lines A S, B S, will deſcribe a Line of the Order $n - 1$; and the manifold Curve will have the Point $n - 2$ in the Pole A or B, as it has been deſcribed by the Interſection of the right Line A S or B S. The Points O, P, Q, W, X, Y, *&c.* will deſcribe Lines of the 2d, 3d, Fig. 13.

4th,

4th, 5th, 6th, 7th, *&c.* Order, but if all the Poles A, B, C, D, E, F, G, H, *&c.* are placed in the fame right Line, thofe Points, O, P, Q, W, X, Y, *&c.* will alfo defcribe as many right Lines.

Fig. 14. The *Newtonian* defcription is alfo greatly promoted by this Method. It is well known, that, if the given Angles O A N, O B N revolve about the given Points A B and the Interfection N of the Legs A N, B N, is drawn through the right Line N R, given by Pofitions, the Interfection O of the Legs A O, B O will defcribe a Conic Section. Now let another Point C be taken, about which let the right Line O C P, be moved, which fhall always pafs through the Interfection O of the Legs A O, B O, and meet the other Leg A N of the Angle A in P; the Interfection P will defcribe a Line of the 3d Order paffing doubly through the Pole A. And in like manner, if by the Interfection B N of the Angle N a Curve is defcribed, it will be of the fame Order, and have a *punctum duplex* in the Pole B. And hence alfo it appears, how a Line of the third Order may be defcribed, through feven given Points, one of which is double.

Fig. 15. Let the Angles O A N, O B N, be moved, as before, about the given Points A, B, and through the Interfection O of the Sides O A, O B, let the right Line O C P pafs, being drawn from another given one C, meeting the Side A N of the Angle A in P; then through P and a 4th given one D, drawn the right Line D P Q meeting the Leg A O in Q; the Point Q will defcribe a Line of the 4th Order, with a *punctum triplex* in the Pole A.

And thus, by increafing the Number of the Poles A, B, C, D, *&c.* fo that their Number at Length may be *n*, the Line defcribed will be of the fame Order *n*. But it fhould be obferved, that for the Angle O B N we fubftitute the right Line, which revolves about the Pole B, the Defcription will be more eafy.

Concerning the Defcription of Curve Lines, *by Mr* Colin McLaurin, Math. Prof. Edinburgh, *F. R. S. Communicated* Dec. 21, 1732.

2. I am informed that fome Papers * have been prefented to the Royal Society of late, concerning the Defcription of Curves, in a manner that has a near Affinity to that which I communicated to them of old, and have carried farther fince; and that it would not be unfeafonable, nor unacceptable, if I fhould fend an Account of what I have done further on that Subject fince the Year 1719. The Author of thofe Papers taught Mathematicks here privately for fome Years, and fome time ago (*viz.* in 1727.) mentioned to me fome Theorems he had on that Subject; which, at the fame Time, I fhewed him in my Papers. Some Time before that, he fhewed me a Theorem which coincided with one of thofe in my Book, tho' he feemed not to have obferved that Coincidence; and indeed Methods of that kind are often found coincident that do not appear fuch at firft Sight. I am unwilling to be the Occafion of difcouraging any thing that is truly ingenious,

* The Papers here hinted at are printed in a Treatife, intituled, *Exercitatio Geometrica de Defcriptione Curvarum.* Authore *Gulielmo Braikenridge*, Lond. 1733. 4*to.*

2 and

and renounce any Pretenſions of appropriating Subjects to myſelf; but, on the contrary, wiſh Juſtice may be done to every Perſon, or to any Performance in Proportion to it's Merit; yet I find it is fit I ſhould take Precautions leſt any one ſhould take it in his Head afterwards to ſay, I take Things from him which I may have had long before him; and therefore ſhall ſend you an Abſtract of what I have done in Relation to this Matter, ſince the Year 1719.

I have ſo much on this Subject by me, that I am at a Loſs what to ſend; but at preſent I ſhall only give you an Abſtract of thoſe Propoſitions, which I take to be more nearly related to thoſe which this Author has offered to the Society from the Converſations I had with him. In 1721, I printed ſeveral Sheets of a Supplement to my Book on the Deſcription of Curve Lines, which I have never yet publiſhed, having been engaged for the moſt part in Buſineſs of a different Nature, and in Purſuits on other Subjects ſince that time. I ſhall firſt give you an Abſtract of that Supplement, as far as it was then printed, and ſhall ſubjoin to this an Account of ſome Theorems I added to it the following Year, *viz.* in 1722. I was led into thoſe new Theorems by Mr *Robert Sympſon*'s giving me at that Time a Hint of the ingenious Paper which has been ſince publiſhed in the Philoſophical Tranſactions. I had tried in the Year 1719, what could be done by the Rotation of Angles on more than two Poles; and had obſerved, that if the Interſections of the Legs of the Angles were carried over right Lines, as in Sir *I. Newton*'s Deſcription, the Dimenſions of the Curve were not raiſed by this Increaſe of the Number of Poles, Angles, and right Lines; and therefore neglected this at that Time, as of no Uſe to me; confining myſelf to two Poles only, and varying the Motions of the Angles as you find them in my Book. I found this by inquiring in how many Points the Locus could cut a right Line drawn in it's Plane, and found, by a Method I often uſe in my Book, that it could meet it in 2 Points only.

Having found then, that 3 or more Poles, were of no more Service than 2, while the Interſections were carried over fixed right Lines, I thought it needleſs to proſecute that Matter then, ſince by increaſing the Number of Poles, my Deſcriptions would become more complex without any Advantage. But in *June* or *July* 1722, upon the Hint I got from Mr *Sympſon* of *Pappus*'s Poriſms, I ſaw that what he has there ingeniouſly demonſtrated, might be conſidered as a Caſe of the abovementioned Deſcription of a Conic Section, by the Rotation of any Number of Angles about as many Poles; the Interſections of their Legs, in the mean time, being carried over fixed right Lines, excepting that of two of them which deſcribes the Locus. For by ſubſtituting right Lines in place of the Angles, in certain Situations of the Poles and of the fixed right Lines, the Locus becomes a right Line; as for Example, in the Caſe of 3 Poles, when theſe 3 are in one right Line, in which Caſe the Locus is a right Line, which is a Caſe of the Poriſm.

This

This led me to confider this Subject anew; and firft I demonftrated the Locus to be a Conic Section algebraically; and found Theorems for drawing Tangents to it, and determining it's Afymptotes. I alfo drew from it at that Time a Method of defcribing a Conic Section thro' 5 given Points*. This encouraged me to fubftitute Curves for the right Lines, to fee if by this Method I could be enabled to carry on my Theorems about the Defcriptions of Lines through given Points to the higher Orders of Lines. Some of the Theorems I found at that Time, I now fend you. In *Nov.* 1722, looking into Sir *Ifaac*'s *Principia*, I faw that the Defcription of the Conic Section by 3 right Lines, moving as above, about 3 Poles, could be immediately drawn from his 20th Lemma, which itfelf is a Cafe of this Defcription. This gradually led me to feek Geometrical Demonftrations for the whole, as far as it related to the Conic Sections. I fend you fome Leaves of this Paper dated at *Nancy*, *Nov.* 1722. Since that Time, I have not added much to this Subject, but what relates to the drawing Tangents, determining the Afymptotes, and the *Puncta Duplicia*, or *Multiplicia* of thefe Curves. I confidered it the lefs, that I did not find it more advantageous in any Refpects, than the Method I had confidered in my Book, or more general.

In 1727 I added to a Chapter in my Algebra, which is very public in this Place, an Algebraic Demonftration of the Locus, when three Poles are employed; and the Method of defcribing a Conic Section through 5 given Points, fubjoining at the fame Time, that if more Poles are employed, and Angles or right Lines, the Locus was ftill a Conic Section; which I thought was a remarkable Property of the Conic Sections not obferved before.

Thefe Things I intended to put in order, and publifh in the Supplement to my Book, a Part of which has been printed fince the Year 1721. I have in my View alfo to give feveral other Things in that Supplement; two of which, I fhall only juft mention at prefent, becaufe I believe they are foreign to the prefent Affair. I fubjoin a Problem determining the Figure of a Fluid, whofe Parts are fuppofed to be attracted to two or more Centers; and a Solution of a general Problem about the Collifion of Bodies.

The Author of the Papers given in to the Royal Society, will not refufe that I fhewed him the Theorems I now fend you, in 1727. He owned it laft Summer at leaft: I am to publifh thefe very foon. Whether he has carried the Subject farther, I leave to the Judgment of the Gentlemen to whom they were referred. As to the Demonftrations, it would take fome Time to put them in a proper Form to be publifhed. I could fend thofe that are algebraic eafily; but do not care to fend thofe that are geometrical, till I have Leifure.

* *The Paper on this Subject I have, is dated* July 31, 1722, *at Sea, being then in my Way to* London, *going for* Cambray.

In

In the firft Part of the Supplement, there is a general Demonftration given of the Theorem, that if two Lines of the Orders or Dimenfions, expreffed by the Numbers m and n, be defcribed in the fame Plane, the greateft Number of Points in which thefe Lines can interfect each other, will be $m\,n$, or the Product of the Numbers which exprefs the Dimenfions of the Lines, or the Orders to which they belong.

An Abftract of what has been printed fince the Year 1721, as a Supplement to a Treatife concerning the Defcription of Curve Lines, publifhed in 1719, and of what the Author propofes to add to that Supplement.

In the next Part, Theorems are given for drawing Tangents to all the Curves that were defcribed in that Treatife by the Motions of Angles upon given Lines. Their Afymptotes are alfo determined by more fimple Conftructions than thofe which are fubjoined to their Defcriptions in that Treatife. Of thefe we fhall give one Inftance here.

Suppofe the invariable Angles F C G, K S H, to revolve about the fixed Points or Poles, C and S. Suppofe the Interfection of the two Sides C F, S K, to be carried over the Curve B Q M, whofe Tangent at the Point Q is fuppofed to be the Right Line A E; and let it be required to draw a Tangent at P to the Curve Line defcribed by P the Interfection of the other two Sides C G and S H. Fig. 16, 17.

Conftruction. Draw Q T conftituting the Angle S Q T, equal to C Q A, on the oppofite Side of S Q, that Q A is from C Q; and let Q T meet C S (produced if neceffary) in T. Join P T, and conftitute the Angle C P N equal to S P T, on the oppofite Side of C P, that P T is from S P, and the Right Line P N, fhall be a Tangent at P, to the Curve defcribed by the Motion of P, which is always fuppofed to be the Interfection of C G and S H.

The Afymptotes of the Curve defcribed by P, are determined thus. Find, as in the abovementioned Treatife, when thefe Sides become parallel, whofe Interfection is fuppofed to trace the Curve; which always happens when the Angle C Q S becomes equal to the Supplement of the Sum of the invariable Angles F C G, K S H, to four Right ones, becaufe the Angle C P S then vanifhes. Suppofe that when this happens, the Interfection of the Sides C F, S K is found in Q. Fig. 18, 19.

Conftitute the Angle S Q T equal to C Q A, as before, and let Q T meet C S in T. Take C N equal to S T, the oppofite Way from C that S T lies from S. Through N draw D N parallel to C G or S H, which are now parallel to each other, and D N fhall be an Afymptote of the Curve defcribed by the Motion of P.

If in place of a Curve Line B Q M, a fixed Right Line A E be fubftituted, then the Point P will defcribe a Conick Section, whofe Tangents and Afymptotes are determined by thefe Conftructions. In this Supplement, it is afterwards fhewn how to draw the Tangents and Afymptotes of all the Curves which are defcribed in the above-mentioned Treatife by more Angles and Lines.

The fame Method is afterwards applied for to draw Tangents to Lines defcribed by other Motions than thofe which are confidered in that Treatife; of which the following is an Inftance. Suppofe that the

Lines C P and S P revolve about the Poles C and S, so that the Angle A C P bears always the same invariable Proportion to A S P, suppose
Fig. 20. that of m to n. In the Line C S, take the Point T, so that S T may be to C T in that same Proportion of m to n; and this Point T will be an invariable Point, since C S is to C T, as $m-n$ to n. Draw T P, and constitute the Angle S P N, equal to C P T, so that P N and P T, may lie contrary ways from S P and C P, and P N shall be a Tangent of the Curve described by the Motion of the Point P. Several other Theorems of this kind are subjoined here.

After these, Lines or Angles are supposed to revolve about three or more Poles, and the Dimensions of the Curves with their Tangents and Asymptotes are determined. Suppose in the first Place, that the three
Fig. 21. Poles are C, S, and D, and that Lines or Rulers C R, S Q, Q D R, revolve about these Poles. The Line which revolves about D, serves only to guide the Motion of the other two, so that it's Intersection with each of them being carried over a fixed Right Line, their Intersection with each other describes the Locus, which is shewn to be a Conick Section. the Intersection of Q D R with S Q, is supposed to be carried over the fixed Right Line A F; the Intersection of the same Q D R, with C R, is supposed to be carried over the fixed Right Line A E; and in the mean time, the Intersection of the Right Lines S Q, CR, that revolve about the Poles S and C, describes a Conick Section.

This Conick Section passes through the Poles C and S; and if you produce D C and D S, till they meet with A Q and A R in F and E, it will also pass through F and E: It also passes always through A, the Intersection of the fixed Lines Q F and E R; from which this easy Method follows for drawing a Conick Section through 5 given Points. Suppose that these 5 given Points are A, F, C, S and E: Join 4 of them by the Lines A F, F C, A E, E S, and produce 2 of these F C, E S, till they meet, and by their Intersection give the Point D. Suppose infinite Right Lines revolve about this Point D, and the Points C and S, two of those that were given, and let the Intersections of the Line revolving about D, with those that revolve about C and S, be carried over the given Right Lines A E, A F; and the Intersection of those that revolve about C and S with each other, will, in the mean Time, describe a Conick Section, that shall pass through the five given Points A, F, C, S and E.

Fig. 22. It is then shewn, that when C, S and D are taken in the same Right Line, the Point P describes a Right Line; as also when C, S and A are in the same Right Line; which also follows from what is demonstrated in that very ingenious Paper concerning *Pappus's* Porisms,
Vid. Vol. VI. p. 76. communicated by Mr *Sympson*, Professor of Mathematicks at *Glasgow*.

In the next Place it is shewn, that if four Right Lines revolve about four Poles C, S, D, and E, and those that revolve about D and E, serve only to guide those that revolve about C and S; so that Q and R, the Intersections of that which revolves about D, with those that revolve

about

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Fig. 12.

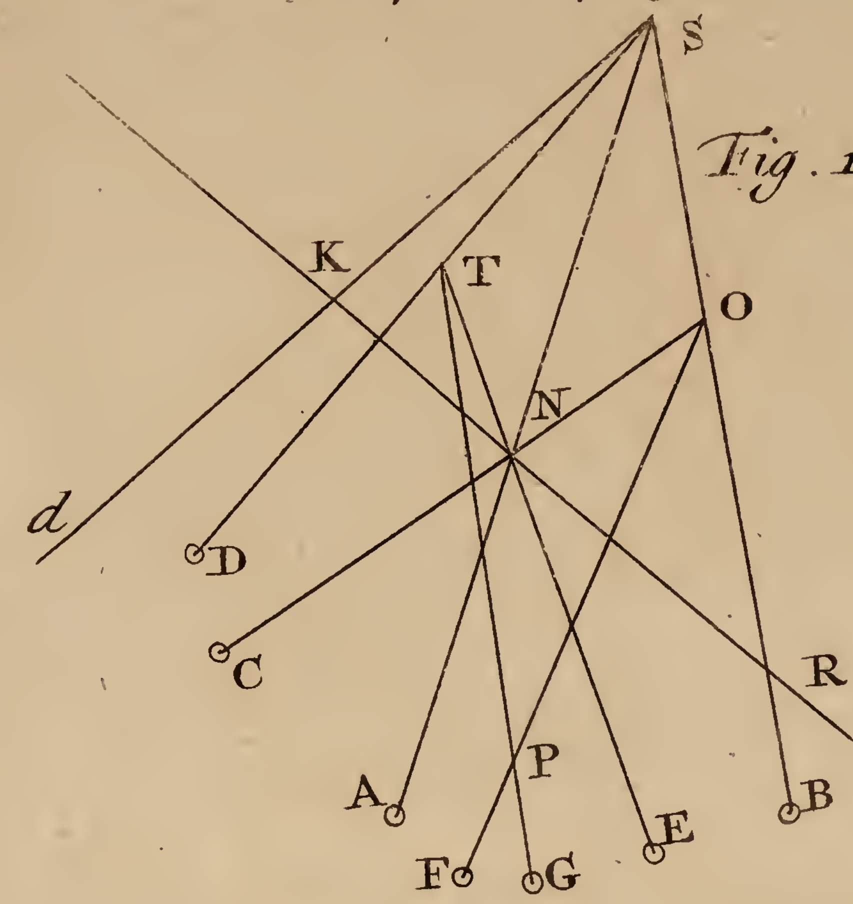


Fig. 13.

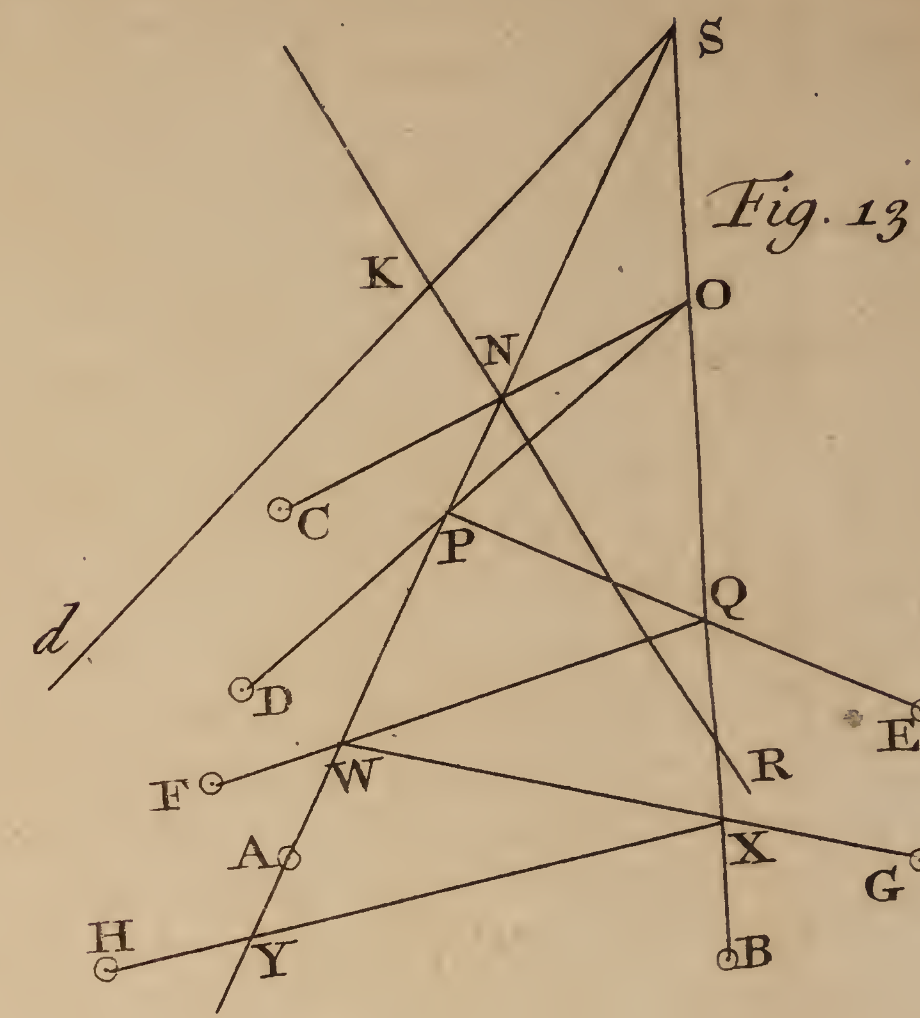


Fig. 14.

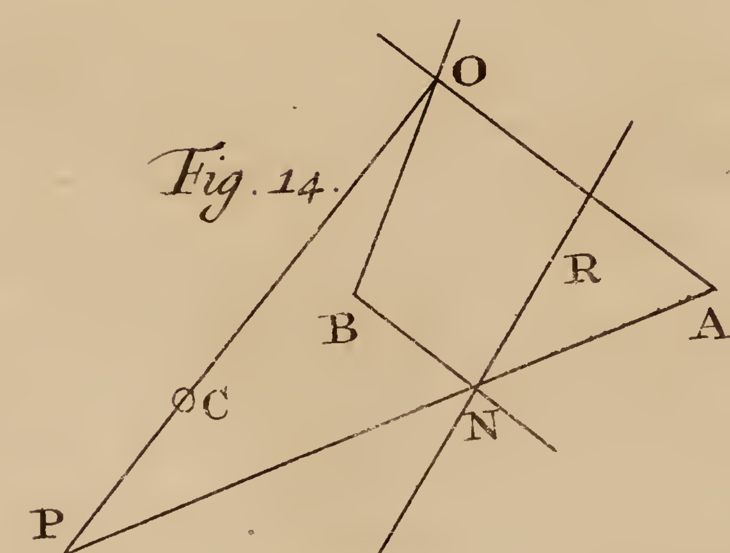


Fig. 15.

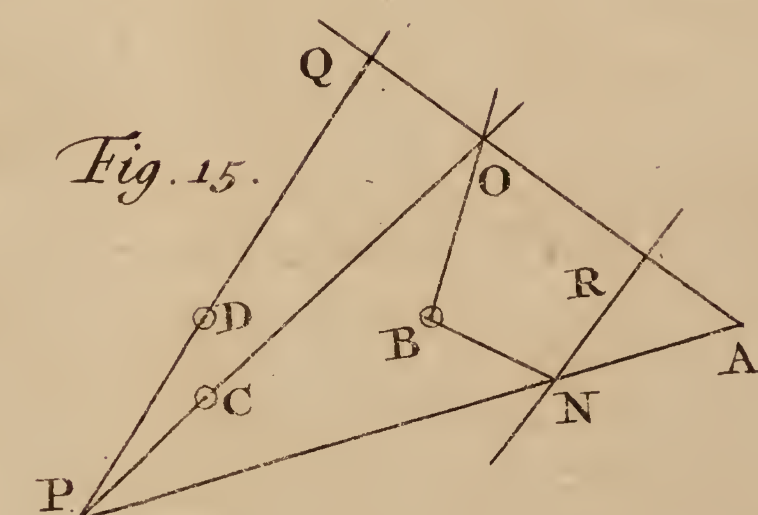


Fig. 16.

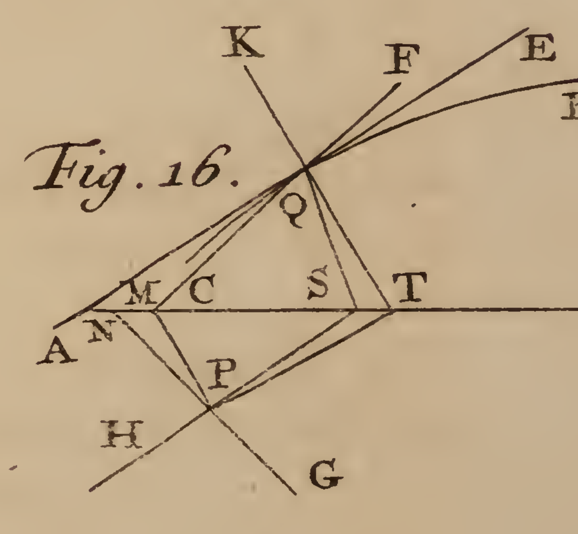


Fig. 17.

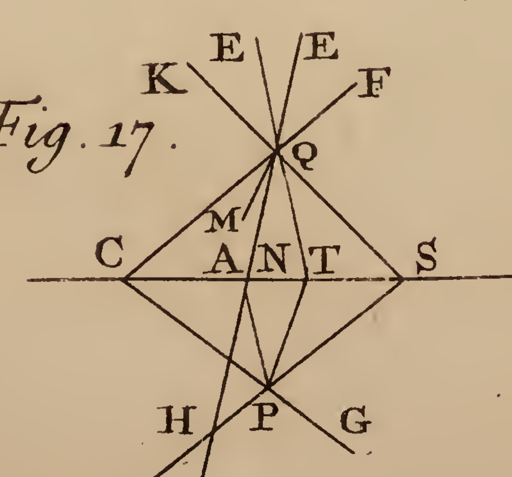


Fig. 18.

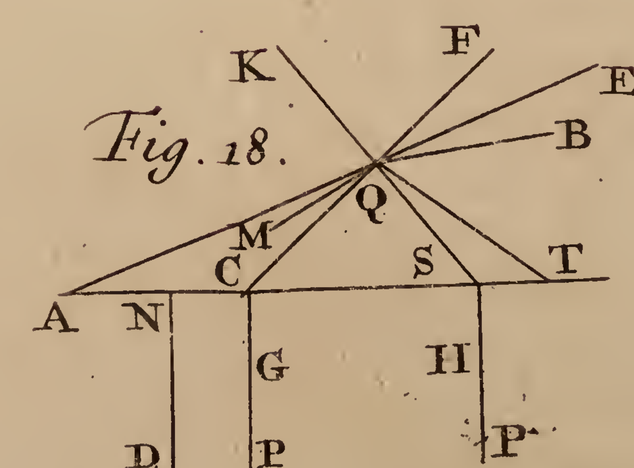
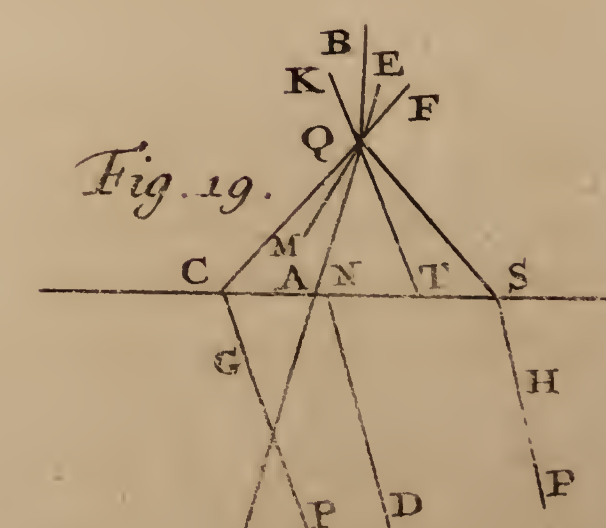


Fig. 19.



about E and S, be carried over the fixed Lines A B and A F; and M the Interfection of that which revolves about E with that which revolves about C, be carried over a third fixed Line B F, then the Interfection P of thofe that revolve about C and S, will in the mean time, defcribe a Conick Section, and not a Curve of a higher Order. The Conick Section degenerates into Right Lines, when C P and S P coincide at the fame time with the Line C S, that joins the Poles C and S, as in the preceding Defcription; which coincides again with what is demonftrated in the abovementioned ingenious Paper.

After this it is fhewn generally, that tho' the Poles and Lines revolving about them be increafed to any Number, and the fixed Lines over which fuch Interfections, as we defcribed in the two laft Cafes, are fuppofed to be carried, be equally increafed, the Locus of the Point P will never be higher than a Conick Section: That is, let a Polygon of any number of Sides have all it's Angles, one only excepted, carried over fixed Right Lines, and let each of it's Sides produced, pafs through a given Point or Pole, and that one Angle which we excepted, will either defcribe a ftreight Line, or Conick Section.

Thus if a hexagonal Figure L Q R P M N, have all it's Angles excepting P carried refpectively over the fixed Right Lines A *a*, B *b*, G *g*, H *h*, K *k*, the Point P in the mean time will defcribe a Conick Section, or a Right Line. The Locus of P is a Right Line when C P and S P coincide together with the Line C S. All thefe things are demonftrated geometrically. Fig. 23.

After this, Angles are fubftituted in place of Right Lines revolving about thefe Poles; and it is ftill demonftrated geometrically, that the Locus of P is a Conick Section or Right Line.

Suppofe that there are 4 Poles C, S, D and E, about which the invariable Angles PCQ, P S R, R D M, M E Q revolve; and that Q, M and R, the Interfections of the Legs C Q and E Q, of E M and D M, and of D R and S R, are carried over the fixed Right Lines A *a*, B *b*, and G *g* refpectively, then the Locus of P is a Conick Section, when C P and S P do not coincide at once with the Line C S, but is a Right Line when C P and S P coincide at the fame time with C S, and never a Curve of a higher Order. Fig. 24.

Having demonftrated this which feems a remarkable Property of the Conick Sections or Lines of the Second Order; I proceed to fubftitute Curve Lines in place of Right Lines in thefe Defcriptions, (as I always do in the Treatife concerning the Defcription of Lines) and to determine the Dimenfions of the Locus of P, and to fhew how to draw Tangents to it to determine it's Afymptotes, and other Properties of it. I had obferved in 1719, that by increafing the Number of Poles and Angles beyond two, the Dimenfions of the Locus of P, did not rife above thofe of the Lines of the Second Order, while the Interfections moved on Right Lines; and therefore I did not think it of ufe to me then to take more Poles than two, fince by taking more, the Defcriptions became

more complex without any Advantage. When the Interſections are carried over Curve Lines, the Dimenſions of the Locus of P riſe higher, but the Curves deſcribed, have *Puncta Duplicia*, or *Multiplicia*, as well as when two Poles only are aſſumed; and therefore this Speculation is more curious than uſeful. However, I ſhall ſubjoin ſome of the Theorems that I found on this Subject concerning the Dimenſions of the Locus of P, and the drawing Tangents to it.

Fig. 21. 1. If you ſuppoſe Q and R to be carried over Curve Lines of the Dimenſions m and n reſpectively, then the Point P may deſcribe a Locus of $2\,mn$ Dimenſions.

Fig. 23. 2. If you ſuppoſe L, Q, R, M, N, to be carried over Curve Lines of the Dimenſions m, n, r, s, t, reſpectively, the Locus of P may ariſe to $2\,mnrst$ Dimenſions, but no higher; and if in place of Lines revolving about the Poles, you uſe invariable Angles, the Dimenſions of the Locus of P will riſe no higher.

Fig. 25. 3. I then aſſumed three Poles C, D and S, and ſuppoſed one of the Angles S N L, to have it's angular Point N carried over the Curve A N, while the Leg N Q paſſes always through S, as in the Deſcription in the Treatiſe of the General Deſcription of Curve Lines, while the Angles Q D R, R C P, revolve about the Poles D and C: I ſuppoſe alſo the Interſections Q and R to be carried over the Curve Lines B Q, G R, and that the Dimenſions of the the Curve Lines A N, B Q, G R, are m, n, r, reſpectively; and find that the Locus of P may be of $3\,mnr$ Dimenſions; but that the Point C is ſuch, that the Curve paſſes through it as often as there are Units in $2\,nmr$.

4. If any number of Poles are aſſumed, ſo as to have Angles revolving about them, as about C and D in the laſt Article, and the Interſections are carried over other Curves, the Dimenſions of the Locus of P will be equal to the triple Product of the Number of Dimenſions of all the Curves employed in the Deſcription.

Fig. 26. 5. If the invariable Angles P N R, P M Q, move ſo that while the Sides P N, P M, paſs always through the Poles C and S, the angular Points N and M deſcribe the Curves A N and B M; and at the ſame time, the invariable Angle R D Q, revolve about the third Pole D, ſo that the Interſections R and Q deſcribe the Curves E R and G Q; then the Dimenſions of the Locus of P, when higheſt, ſhall be equal to the quadruple Product of the Numbers that expreſs the Dimenſions of the given Curves A N, E R, G Q and B M, multiplied continually into each other. If more Poles are aſſumed, about which Angles be ſuppoſed to move, as R D Q moves about D in this Deſcription, and the Interſections of the Sides be ſtill carried over Curves, as in this Example; the Dimenſions, of the Locus of P, when higheſt, ſhall ſtill be found equal to the quadruple Product of all the Numbers that expreſs the Dimenſions of the Curves employed in this Deſcription.

Fig. 27. 6. Suppoſe that the three invariable Angles P Q K, K L R, R N P, move over the Curves G Q, E L, A N, ſo that the Sides P Q, K L, P N produced,

produced, pass always through the Poles C, D, S, and that the Intersections of their Sides K and R, at the same time move over the Curves F K and B R; and the Dimensions of the Locus of P when highest, shall be equal to the Product of the Numbers that express the Dimensions of the given Curves multiplyed by 6. If more Poles, with the necessary Angles and Curves, are assumed betwixt C and D, as here D is assumed betwixt C and S, and the Motions be in other respects like to what they are in this Example; then in order to find the Dimensions of the Locus of P when highest, raise the Number 2 to a Power whose Index is less than the Number of Poles by a Unit; add 2 to this Power, and multiply the Sum by the Product of the Numbers that express the Dimensions of the Curves employed in the Description; and this last Product shall shew the Dimensions of the Locus of P when highest.

I am able to continue these Theorems much farther: But it is not worth while, especially since I find that there is not any considerable Advantage obtained by increasing the number of Poles above the Method delivered in the abovementioned Treatise of the Description of Curve Lines. On the contrary, the Descriptions there given by means of two Poles, will produce a Locus of higher Dimensions by the same number of Curves and Angles, than these that require three or more Poles; and are therefore preferrable, unless perhaps in some particular Cases.

However, I have also found how to draw Tangents to the Curves that arise in all these Descriptions; of which I shall give one Instance where 3 Right Lines are supposed to revolve about 3 Poles, and 2 of their Intersections are supposed to be carried over given Curve Lines, and the third describes the Locus required.

Let the Right Lines C Q, S N, D N, revolve about the Poles C, S, D, Fig. 28.
where that which revolves about D, serves to guide the Motion of the other two; it's Intersection with C Q moving over the Curve G Q, while it's Intersection with S N moves over the Curve F N. Suppose that the Right Line B *b* touches the Curve G Q in Q, and that the Right Line A *a* touches the Curve F N in N. In order to draw a Tangent to the Locus of P; join D C, D S and C S, and constitute the Angle D Q R, equal to C Q B, so that Q R lie the contrary way from Q D that Q B lies from Q C, and let Q R meet D C in R. Constitute also the Angle D N T, equal to S N A with the like precaution, and let N T meet D S in T. Join R T, and produce it till it meet C S in H; then join P H, and make the Angle C P L equal to S P H, so that P L and P H, may lie contrary ways from C P and S P; and P L shall be a Tangent at P, to the Locus described by P, the Intersection of C Q and S N.

I have also applied this Doctrine to the Description of Lines through given Points. But I suppose I have said enough at present on this Subject; and shall conclude, after observing that in the abovementioned Treatise, I have given an easy Theorem for calculating the Resistance of the Medium when a given Curve is described with a given centripetal

Force in a refifting Medium, which I fhall here repeat, becaufe it has been mifreprefented in a foreign Journal.

Let V exprefs the centripetal Force with which the Body that is fuppofed to defcribe the Curve, is acted on in the Medium; let v exprefs the centripetal Force with which the fame Curve could be defcribed in a Void; fuppofe $z = \frac{V}{v}$, and the Refiftance fhall be proportional to the Fluxion of z multiplied by the Fluxion of the Curve, fuppofing the Area defcribed by a Ray, drawn from the Body to the Center of the Forces, to flow uniformly. Let this Theorem be compared with what the celebrated Mathematician mentioned by that Journalift has given on the fame Subject, and it will eafily appear what judgment is to be made of his Affertion; and fince feveral Perfons, and particularly the Gentleman mentioned above in this Paper, teftify that I communicated to them this Theorem before any Thing was publifhed on this Subject by the learned Mathematician he names, his Obfervation on this Occafion muft appear the more groundlefs.

From this Theorem, I draw this very general Corollary, that if the Curve is fuch as could be defcribed in a Void by a centripetal Force, varying according to any Power of the Diftance, then the Denfity of the Medium in any place, is reciprocally proportional to the Tangent of the Curve at that place, bounded at one Extremity by the Point of Contact, and, at the other, by it's Interfection with a Perpendicular raifed at the Center of the Forces to the Ray drawn from that Center to the Point of Contact. Let A L be the Curve defcribed by a Force directed to the point S; let L T touch the Curve at L, and raife S T perpendicular to S L, meeting L T in T, and the Denfity in L fhall be inverfely as L T, if the Refiftance be fuppofed to obferve the compound Proportion of the Denfity, and of the Square of the Velocity.

Fig. 29.

Befides what I have obferved here, I propofe to illuftrate and improve feveral other Parts of the Treatife concerning the Defcription of Curve Lines in this Supplement.

That Treatife requires thefe Additions and Illuftrations the more, that tho' the whole almoft was new, it was publifhed in a hurry, when I was very young, before I had time to confider fufficiently which were the beft ways of demonftrating the Theorems, or refolving the Problems, for which this Supplement I hope, will make fome Apology.

The Paper dated at Nancy, Nov. 27. 1722, *mentioned in the foregoing article Prop* 1. Sect. I.

3. About the Poles C, B, D, let the Right Lines C d, B m, D r be moved, and let the Interfection of the Legs B m, D r be drawn thro' the given Right Line P G, the Interfection of the Legs C d, D r thro' the given Right Line P Q, and the Interfection of the Legs C d, B d will defcribe a Conick Section.

Fig. 30.

Draw $r t$ parallel to the Right Line B D given by Pofition, and let it meet the Right Line B d in t; join P t and produce it till it meets the Right Line B D in F; and you will have the Point F. For as the

Proportion is given of *r u* to *r t*, which is the same as of D G to D B, because of the similar figures D *m* B G and *r m t u*, and *r u* is to *r t* as Q G to Q F, the Proportion will also be given of Q F to Q G; and so because of the given one Q G, Q F will be given, and therefore the Point F and the Right Line P F. Since therefore B *t* and C *r* cut off the parts P *t*, P *r*, from the Right Lines given by Position P F, P Q, their Intersection *d* will always be in a given *Ratio* in a Conick Section, by *Lem.* 20. *Lib.* 1. *Newt. Princip.*

If the Point D be taken any where in the Right Line B F, and if D G is always to Q G as B D to Q F, the Conick Section will be the same that *d* shall describe.

The Conick Section passes thro' C, P, B, and *a* by compleating the Parallellogram P S *a u*. It also passes thro' L where the Right Line B G being produced meets P *u*, as also thro' K, where the Right Line C D cuts the given one P G. Whence the Pentagon P K C L B is inscribed on the Section. And if 5 points C, K, P, B, L are given, thro' which the Conick Section is to be drawn, or if the Conick Section is to be circumscribed about the given Pentagon C L B P K, let any 2 sides C K, L B be produced to their Intersection D, and then let the rest P L, P K be joined, and let the Intersections of the Right Lines C *d*, D *r*, and B *d*, D R be always drawn thro' those Right Lines P L, P K, and the Intersection *d* will describe the Section.

About the given Points F, C, G, S, as Poles, let the Right Lines F Q, C N, G Q, S L be moved, and let the Intersections of the Right Lines F Q and C N, F Q and G Q, G Q and S L, namely the Points M, Q, L, always touch the Right Lines given by Position A E, B E, H L, and the Intersection of the Right Lines C N, S L, will describe a Conick Section. *Prop.* 2. Fig. 31.

Let the Right Lines A M, H R meet B Q in E and H. Let C F and G S be joined meeting each other in D, let D Q be joined meeting the Right Lines C M, S L in N and R; and if E N and H R are joined, E N and H R will be Right Lines given by Position by *Lemma* I. For as the Points F, C, D are in the same Right Line, and the Intersections of the Right Lines F M, C M, and F Q, D Q run over the given Right Lines, the Intersection of the Legs C M, D Q will also touch the given one. And for the like Reason as S, D, G are in the same Right Line, the Intersection of the Right Lines D Q, S L will also touch the given one.

Therefore omitting the Poles F and G, the Curve is to be found which the Intersection of the Right Lines C N, S L, viz. P will describe, whilst, as the Right Lines C N, D N, S R revolve about the Poles C, D, S, the Intersection of the Right Lines C N, D N touches the given E N, and the Intersection of the Right Lines S R, D N touches the given one H R, and that this is a Conick Section is manifest from the foregoing Proposition.

VII. Ha-

Concerning two Species of Lines *of the* Third Order, (*not mentioned by Sir* I. Newton, *nor Mr* Sterling) *by Mr* Edmund Stone, F.R.S. N°. 456. p. 318. *Jan.* &c. 1740. dated *July* 31. 1736.

VII. Having for some time past been reading and considering the little Treatise of Sir *I. Newton* intituled *Enumeratio Linearum tertii Ordinis*, as also the ingenious Piece of Mr *Sterling* called *Illustratio Tractatus Domini* Newtoni *Linearum tertii Ordinis*; I have observed, that they have neither of them taken Notice of the two following Species of Lines of the Third Order; and venture to affirm, that the 72 Species mentioned by Sir *Isaac*, together with the 4 more of Mr *Sterling*, and these Two, making in all 78, is the exact Number of the different Species of the Lines of the Third Order, according to what Sir *Isaac* has thought fit to constitute a different Species.

The two Species I mean, are to be reckoned amongst the Hyperbolo-parabolical Curves, having one Diameter, and one Asymptote, at N°. 8. of *Newton's* Treatise, or Page 104. of Mr *Sterling*'s; whose Equation is $xyy = \underline{+} bx^2 \underline{+} cx \underline{+} d$; which will give, not 4, as in these Authors, but 6 Species of these Curves: For,

I. If the Equation $bx^2 + cx + d = 0$, has two impossible Roots, the Equation $xyy = bx^2 \underline{+} cx + d$, will (as they say) give two Hyperbolo-parabolical Figures equally distant on each side the Diameter AB. See the 57th Figure in *Newton*'s Treatise, and this is his 53d Species, and *Sterling*'s 57th.

II. If the Equation $bx^2 - cx + d = 0$, has two equal Roots both with the Sign $+$; the Equation $xyy = bx^2 - cx + d$, will (as they say) give two Hyperbolo-parabolical Curves crossing each other at the Point τ in the Diameter. See Fig. the 58th in *Newton*; and this is his 54th Species, and *Sterling*'s 58th.

Fig. 32. III. But if the Equation $bx^2 + cx + d = 0$, has two possible unequal negative Roots Aρ and Aτ, the Curve given by the Equation $xyy = + bx^2 + cx + d$, will consist of two Hyperbolo-parabolical Parts, as also of an Oval on the contrary Side the Asymptote or principal Abscis. And this is one of the Species omitted by Sir *Isaac* and Mr *Sterling*, which is really the 59th Species.

Fig. 33. IV. Also if the Equation $bx^2 + cx + d = 0$, has two equal negative Roots Aρ and Aτ; the Curve given by the Equation $xyy = + bx \underline{+} cx + d$, will consist of two Hyperbolo-parabolical Parts, and also of a Conjugate Point on the contrary Side the Asymptote or principal Ordinate: And this is the other Species of these Curves omitted by Sir *Isaac* and Mr *Sterling*, which is really the 60th Species.

V. If the Roots of the Equation $bx^2 - cx + d = 0$ are real, and unequal, having both the Sign $+$; the Curve given by the Equation $xyy = bx^2 - cx + d$, will (as they say) consist of a conchoidal Hyperbola and a Parabola, on the same side the Asymptote or principal Ordinate. See Fig. the 59th in *Newton*; and this is really the 61st Species.

VI. If the Roots of the Equation $bx^2 \underline{+} cx - \overline{d} = 0$, have contrary Signs, the Equation $xyy = bx^2 \underline{+} cx - \overline{d}$, will (as they say) give a conchoidal

conchoidal *Hyperbola* with a *Parabola* on the contrary Side the Afymptote or principal Ordinate. See Fig. the 60th in *Newton*; and this is really the 62d Species.

VIII. Many Attempts have been made at different times, but, if I miftake not, never any yet with tolerable Succefs, towards the Solution of the Problem propofed by *Kepler*: To divide the Area of a Semicircle into given Parts, by a Line from a given Point of the Diameter, in order to find an univerfal Rule for the Motion of a Body in an Elliptic Orbit. For among the feveral Methods offered, fome are only true in Speculation, but are really of no Service. Others are not different from his own, which he judged improper: And as to the reft, they are all fome way or other fo limited and confined to particular Conditions and Circumftances, as ftill to leave the Problem in general untouched. To be more particular; it is evident, that all Conftructions by Mechanical Curves are feeming Solutions only, but in reality unapplicable; that the Roots of infinite Series's are, upon account of their known Limitations in all refpects, fo far from affording an Appearance of being fufficient Rules, that they cannot well be fuppofed as offered for any thing more than Exercifes in a Method of Calculation. And then, as to the univerfal Method, which proceeds by a continued Correction of the Errors of a falfe Pofition, it is, when duly confidered, no Method of Solution at all in itfelf; becaufe unlefs there be fome antecedent Rule or Hypothefis to begin the Operation, (as fuppofe that of an uniform Motion about the upper Focus, for the Orbit of a Planet; or that of a Motion in a Parabola for the perihelian Part of the Orbit of a Comet; or fome other fuch) it would be impoffible to proceed one ftep in it. But as no general Rule has ever yet been laid down, to affift this Method, fo as to make it always operate, it is the fame in Effect as if there were no Method at all. And accordingly in Experience it is found, that there is no Rule now fubfifting but what is abfolutely ufelefs in the Elliptic Orbits of Comets; for in fuch Cafes there is no other way to proceed but that which was ufed by *Kepler*: To compute a Table for fome part of the Orbit, and therein examine if the Time to which the Place is required, will fall out any-where in that Part. So that, upon the whole, I think, it appears evident, that this Problem (contrary to the received Opinion) has never yet been advanced one Step towards it's true Solution: A Confideration which will furnifh a fufficient Plea for meddling with a Subject fo frequently handled; efpecially if what is offered fhall at the fame time appear (as I truft it will) to contribute towards fupplying the main Defect.

The Solution of Kep'er's Problem, *by* J. Machin, *Aftr. Prof.* Grefh. *and Secr.* R. S. N°. 447. p. 205. Jan. &c. 1738.

The Tangent of an Arch being given, to find the Tangent of it's Multiple. Lemma I.

Let r be the Radius of the Circle, t the Tangent of a given Arch A, and n a given Number. And let T be the Tangent of the Multiple Arch $n \times A$ to be found.

Then if $\varrho\varrho$ be put for $-rr$, and $\tau\tau$ for $-tt$;

The Tangent T will be $\dfrac{\overline{r+\tau}|^n - \overline{r-\tau}|^n}{\overline{r+\tau}|^n + \overline{r-\tau}|^n}\varrho$: Which Binomials being raised according to Sir *I. Newton*'s Rule, the fictitious Quantities τ and ϱ will disappear, and the Tangent T will become equal to

$$\frac{nt - \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}.\frac{t^3}{r^2} + \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}\frac{n-3}{4}.\frac{n-4}{5}.\frac{t^5}{r^4} - \&c.}{1 - \frac{n}{1}.\frac{n-1}{2}.\frac{tt}{rr} + \frac{n}{1}.\frac{n-1}{2}.\frac{n-2}{3}.\frac{n-3}{4}.\frac{t^4}{r^4} - \&c.}$$

This Theorem (which I formerly found for the Quadrature of the Circle, at a time when it was not known here to have been invented before) has now been common for many Years; for which Reason I shall premise it, at present, without any Proof; only for the sake of some Uses that have not yet been made of it.

Corol. 1. From this Theorem for the Tangent, the Sine (suppose) Y, and Cosine Z of the Multiple Arch $n \times$ A, may be readily found.

For if y be the Sine, and z the Cosine of the given Arch A, then putting vv for $-yy$, and substituting $\frac{ry}{z}$ for t, and $\frac{rv}{z}$ for τ, and $\frac{rT}{\sqrt{rr+TT}}$ for Y: The Sine Y will be $\dfrac{\overline{z+v}|^n - \overline{z-v}|^n}{2r^n}\varrho$. The Cosine Z will be $\dfrac{\overline{z+v}|^n + \overline{z-v}|^n}{2r^{n-1}}$.

Each of these may be expressed differently in a Series, either by the Sine and Cosine conjointly, or by either of them separately.

Thus Y *the Sine* of the multiple Arch $n \times$ A, may be in either of these two Forms, *viz.*

$$= \frac{z^{n-1}}{r^{n-1}} y \text{ in } n - \frac{n-1}{2}.\frac{n-2}{3}\overset{1}{A}.\frac{y^2}{z^2} + \frac{n-3}{4}.\frac{n-4}{5}\overset{1}{B}.\frac{y^4}{z^4} - \&c.$$

$$\text{or} = ny - \frac{nn-1}{2.3rr}\overset{1}{A}y^3 - \frac{nn-9}{4.5rr}\overset{1}{B}y^5 - \frac{nn-25}{6.7rr}\overset{1}{C}y^7 - \&c.$$

Wherein the Letters $\overset{1}{A}$, $\overset{1}{B}$, $\overset{1}{C}$, *&c.* stand, as usual, for the Coefficients of the preceding Terms.

The first of these Theorems terminates when n is any integer Number, the other (which is Sir *I. Newton*'s Rule, and is derived from the former by substituting $\sqrt{rr-yy}$ for z) terminates when n is any odd Number.

The *Cosine* Z may, in like manner, be in either of these two Forms *viz.*

$$=\frac{z^n}{r^{n-1}} \text{ in } 1 - \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{y^2}{z^2} + \frac{n}{1}\cdot\frac{n-1}{2}\cdot\frac{n-2}{3}\cdot\frac{n-3}{4}\cdot\frac{y^4}{z^4} - \&c.$$

$$\text{or} = r - \frac{nn}{2rr}Ay^3 - \frac{nn-4}{3\cdot 4rr}By^4 - \frac{nn-16}{5\cdot 6rr}Cy^6 - \&c.$$

The latter of which terminates when the Number n is even, and the other as before, when it is any Integer.

Corol. 2. Hence the Sine, Cofine, and Tangent of any Submultiple Part of an Arch (*fuppofe*) $\frac{1}{n}A$, may be determined thus:

The Tangent of $\frac{1}{n}A$ will be $\dfrac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}\varrho$

The Sine of $\frac{1}{n}A$ will be $\dfrac{\overline{z+v}|^{\frac{1}{n}} - \overline{z-v}|^{\frac{1}{n}}}{2r^{\frac{1}{n}}}\varrho$

For thefe Equations will arife from the Tranfpofition and Reduction of the former for the Tangent and Sine of the Multiple Arch, upon the Subftitution of t, y, z and A; for T, Y, Z and $n \times A$.

Corol. 3. Hence regular Polygons of any given Number of Sides may be infcribed within, or circumfcribed without, a given Arch of a Circle. For if the Number n exprefs the double of the Number of Sides to be infcribed within, or circumfcribed about, the given Arch A; then one of the Sides infcribed will be the double of the Sine, and one of the Sides circumfcribed the double of the Tangent of the Submultiple Part of the Arch, *viz.* $\frac{1}{n}A$.

Lemma II. *To find the Length of the Arch of a Circle within certain Limits, by means of the Tangent and Sine of the Arch.*

Let t be the Tangent, y the Sine and z the Cofine of the Arch A, whofe Length is to be determined, and let ϱ, τ, v be expounded as before; then, if any Number n be taken, the Arch of the Circle will be always lefs than $\dfrac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}} \times n\varrho$, and bigger than $\dfrac{\overline{z+v}|^{\frac{1}{n}} - \overline{z-v}|^{\frac{1}{n}}}{2r^{\frac{1}{n}}} \times n\varrho$.

For if, by the preceding *Corollaries*, a regular rectilinear Polygon be infcribed within, and another without, the Arch A, each having half fo many Sides as is expreffed by the Number n; then will the

former of these Quantities be the Length of the Bow of the circumscribed Polygon, (or the Sum of all it's Sides) which is always bigger and the latter will be the Length of the Bow of the inscribed Polygon, which is always less, than the Arch of the Circle; how great soever the Number n be taken.

Corol. 1. Hence the Series's for the Rectification of the Arch of a Circle may be derived.

For by converting the Binomials into the Form of a Series, that the fictitious Quantities, ϱ, τ, v may be destroyed; it will appear, that no Number n can be taken so large as to make the inscribed Polygon so big, or the circumscribed so little as the Series.

$\frac{ry}{z} - \frac{ry^3}{3z^3} + \frac{ry^5}{5z^5} - \frac{ry^7}{7z^7} +$ &c. in one Case, or it's Equal $t - \frac{t^3}{3r^2} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} +$ &c. in the other Case,

Wherefore since the Quantity denoted by the Sum of the Terms in either of these Series's is always bigger than any inscribed Polygon, and always less than any circumscribed, it must therefore be equal to the Arch of the Circle.

Corol. 2. If, in the first of the above Series's, the Root $\sqrt{rr - yy}$, be extracted and substituted for z, there will arise the other Series of Sir *I. Newton*, for giving the Arch from the Sine; namely, $y + \frac{y^3}{6r^2} + \frac{3y^5}{40r^4} + \frac{5y^7}{112r^6} +$ &c. or otherwise, $= y + \frac{1}{1.2.3} \times \frac{y^3}{r^2} + \frac{3.3}{1.2.3.4.5.} \times \frac{y^5}{r^4} + \frac{3.3.5.5}{1.2.3.4.5.6.7.} \times \frac{y^7}{r^6} +$ &c.

Scholium. In like manner, as the Arches of the Polygons serve to determine the Arch of the Circle, so by comparing the Areas of the circumscribed and inscribed Polygons, $\frac{1}{2} n r T$ and $\frac{1}{2} n Y Z$, the Area of the Sector of a Circle may be found. For if T, Y and Z are the Tangent, Sine, and Cosine of the Arch A; then by the second *Lemma* the Area of the circumscribed Polygon will be found to be $\frac{1}{2} n r \varrho \times \frac{\overline{r+\tau}|^{\frac{1}{n}} - \overline{r-\tau}|^{\frac{1}{n}}}{\overline{r+\tau}|^{\frac{1}{n}} + \overline{r-\tau}|^{\frac{1}{n}}}$ $= \frac{1}{2} n r T$. and the Area of the inscribed will appear to be $\frac{1}{2} n \varrho \times$ $\frac{\overline{z+v}|^{\frac{2}{n}} - \overline{z-v}|^{\frac{2}{n}}}{4r^{\frac{2}{n}-1}} = \frac{1}{2} n Y Z.$

But

But upon the Expanſion of theſe Binomials it will appear, that no Number n can be taken ſo large as to make the one ſo big, or the other ſo little, as the Area denoted by the Series. $\frac{1}{2}r$ in $t - \frac{t^3}{3rr} + \frac{t^5}{5r^4} - \frac{t^7}{7r^6} +$ &c.

So that this Area being larger than any inſcribed, and ſmaller than any circumſcribed, Polygon, muſt be equal to the Area of the Sector.

It may further be obſerved, that as the Arch or Area is found from the Sine, Coſine, or Tangent of the Arch, by means of the limiting Polygons, ſo may the Sine, Coſine, or Tangent be found from the Length of the Arch by the ſame Method.

Thus, if A be the Arch whoſe Tangent T, Sine Y, and Coſine Z, are to be determined, then will the

$$\text{Tangent } T \text{ be } = \frac{A - \frac{1}{1.2.3} \times \frac{A^3}{r^2} + \frac{1}{1.2.3.4.5} \times \frac{A^5}{r^4} - \&c.}{1 - \frac{1}{1.2} \times \frac{A^2}{rr} + \frac{1}{1.2.3.4} \times \frac{A^4}{r4} - \&c.}$$

$$\text{Sine } Y = A - \frac{1}{1.2.3} \times \frac{A^3}{r^2} + \frac{1}{1.2.3.4.5} \times \frac{A^5}{r^4} - \&c.$$

$$\text{Coſine } Z = r - \frac{1}{1.2} \times \frac{A^2}{r} + \frac{1}{1.2.3.4} \times \frac{A^4}{r^3} \&c.$$

For it may be made to appear, from the firſt *Lemma*, and it's *Corollaries*, that if in any of theſe Theorems, as ſuppoſe in the Firſt, the Quantity A ſtand for the Bow of the circumſcribed Polygon, then will the Quantity T exhibited by the Theorem, be always bigger; but if for the Bow of the inſcribed, always leſs than the Tangent of the Arch, how great ſoever the Number n be taken; and conſequently, if A ſtand for the Length of the Arch itſelf, the Quantity T muſt be equal to the Tangent; and the like may be ſhewn for the Sine, and *mutatis mutandis*, for the Coſine.

Theſe Principles, from whence I have here derived the Quadrature of the Circle, which is wanted in the Solution of the Problem in hand, happen to be upon another Account abſolutely requiſite for the Reduction of it to a manageable Equation. But I have inlarged, more than was neceſſary to the Problem itſelf, on the Uſes of this ſort of Quadrature by the limiting Polygons, becauſe it is one of that kind which requires no other Knowledge but what depends on the common Properties of Number and Magnitude; and ſo may ſerve as an Inſtance to ſhew that no other is requiſite for the Eſtabliſhment of Principles for Arithmetick and Geometry. A Truth, which though certain in itſelf, may

may perhaps ſeem doubtful from the Nature and Tendency of the preſent Inquiries in Mathematicks. For among the Moderns ſome have thought it neceſſary, for the Inveſtigation of the Relations of Quantities, to have Recourſe to very hard Hypotheſes; ſuch as that of Number infinite and indeterminate; and that of Magnitudes in *Statu fieri*, exiſting in a potential Manner, which are actually of no Bigneſs. And others, whoſe Names are truly to be reverenced on Account of their great and ſingular Inventions, have thought it requiſite to have Recourſe even to Principles foreign to Mathematicks, and have introduced the Conſideration of efficient Cauſes and Phyſical Powers for the Production of Mathematical Quantities; and have ſpoken of them, and uſed them, as if they were a Species of Quantities by themſelves.

N. B. In the following Propoſition I have, for the Sake of Brevity, made uſe of a peculiar Notation for compoſite Numbers (or ſuch Quantities as are analogous to them) whoſe Factors are in Arithmetical Progreſſion.

The Quantity expreſſed by this Notation has a double Index: that as the Head of the Root at the Right-hand, but ſeparated by a Hook to diſtinguiſh it from the common Index, denotes the Number of Factors; and that above, within the Hook on the Left-hand, denotes the common Difference of the Factors proceeding in a decreaſing or increaſing Arithmetical Progreſſion.

Thus the Quantity $\overset{\alpha}{\overline{n+a}}\big(^{m}$ denotes by it's Index *m* on the Right-hand, that it is a compoſite Quantity, conſiſting of ſo many Factors as there are Units in the Number *m*; and the Index α above, on the Left, denotes the common Difference of the Factors, decreaſing in an Arithmetical Progreſſion, if it be poſitive; or increaſing, if it be negative; and ſo ſignifies, in the common Notation, the compoſite Number or Quantity, $\overline{n+a}.\ \overline{n+a-\alpha}.\ \overline{n+a-2\alpha}.\ \overline{n+a-3\alpha}.$ and ſo on.

For Example: $\overset{2}{\overline{n+5}}\big(^{6}$ is $= \overline{n+5}.\ \overline{n+3}.\ \overline{n+1}.\ \overline{n-1}.\ \overline{n-3}.\ \overline{n-5}$, conſiſting of ſix Factors whoſe common Difference is 2. After the ſame Manner $\overset{2}{\overline{n+4}}\big(^{5}$ is $= \overline{n+4}.\ \overline{n+2}.\ n.\ \overline{n-2}.\ \overline{n-4}$, conſiſting of five Factors. According to which Method it will eaſily appear, that if *a* be any Integer, then $\overset{2}{\overline{n+2a+1}}\big(^{2a+2}$ will be

=

$= \overline{nn-1}.\ \overline{nn-9}.\ \overline{nn-25}$, continued to such a Number of double Factors as are expressed by $a+1$, or half the Index, which in this Case is an even Number. So $\overset{2}{\overline{n+2a}}{}^{(2a+1}$ will be equal to $n.\ \overline{nn-4}.\ \overline{nn-16}.\ \overline{nn-36}$, and so on, where there are to be so many double Factors as with one single one (n) will make up the Index $2a+1$, which is an odd Number.

If the common Difference α be an Unit, it is omitted: Thus, $\breve{n}^{(6}$ is $= n.\ \overline{n-1}.\ \overline{n-2}.\ \overline{n-3}.\ \overline{n-4}.\ \overline{n-5}$, containing 6 Factors. So $\breve{6}^{(6}$ is $= 6.\ 5.\ 4.\ 3.\ 2.\ 1$, and the like for others.

If the common Difference α be nothing, then the Hook is omitted, and it becomes the same with the Geometrical Power:

So $\overset{0}{\overline{n+a}}{}^{(m}$ is $= \overline{n+|}^{m}$ according to the common Notation.

An Arch less than a Semicircle being given, with a Point in the Diameter passing through one of it's Extremities; to find, by means of the Sine of a given Part of the Arch less than one half, the Area of the Sector subtended by the given Arch, and comprehended in the Angle made at the given Point. Prop. I.

Let P N A be a Semicircle described on the Centre C, and Diameter A P, and let P N be the given Arch less than a Semicircle, and S the given Point in the Diameter A P passing through one of the Extremities of the Arch N P in P. Then taking any Number n bigger than 2, let P K be an Arch in Proportion to the given Arch P N, as Unity to the Number n; and let it be required to find by means of the Sine of the Arch P K, the Area of the Sector N S P subtended by the given Arch N P, and comprehended in the Angle N S P made at the given Point S.

From N and K let fall on the Diameter A P the Perpendiculars N M and K L, and join C N and C K.

Then let t stand for C P the Semidiameter of the Circle; f for C S, the Distance of the given Point S from the Center; p for S P the Distance of it from the Extremity of the Arch through which the Diameter A P passes; and y for K L the Sine of the Arch K P in the given Circle.

These Substitutions being presupposed, the Problem is to be divided into two Cases; one when S P is less, and the other when it is greater than the Semidiameter C P.

If S P be less than C P, then take an Area H equal to the Sum of the Rectangles expressed by the several Terms of the following Series continued *ad libitum*: Case I.

$\frac{py}{1}$

$$\frac{py}{1} + \frac{t + \overset{2}{\overline{n+1}}|^{2} \times f}{\underset{\smile}{3}|^{3}} \times \frac{)^{3}}{t^{2}} + \frac{9t - \overset{2}{\overline{n+3}}|^{4} \times f}{\underset{\smile}{5}|^{5}} \times \frac{)^{5}}{t^{4}}$$

$$+ \frac{9 \times 25\, t + \overset{2}{\overline{n+5}}|^{6} \times f}{\underset{\smile}{7}|^{7}} \times \frac{)^{7}}{t^{6}} + \text{\&c.}$$ And the Area $\frac{1}{2}$

$n \times H$ will determine the Area of the Sector N S P *ad libitum.*

For the Sector P S N, being the Excess of the Sector N C P above the Triangle N C S, will be the Difference of two Rectangles: $\frac{1}{2}$ C P × P N — $\frac{1}{2}$ C S × N M; but P N is the Multiple of the Arch P K, namely $n \times$ P K; and N M is the Sine of that multiple Arch: Wherefore if for C P be put t, for C S, f, according to the Supposition; and if for P K be substituted: $\frac{y}{1} + \frac{1}{\underset{\smile}{3}|^{3}} \times \frac{)^{3}}{t^{2}} + \frac{9}{\underset{\smile}{5}|^{5}} \times \frac{)^{5}}{t^{4}} + \frac{9 \times 25}{\underset{\smile}{7}|^{7}} \times \frac{)^{7}}{t^{6}} + \text{\&c.}$ by *Cor.* 2. *Lem.* 2; and for N M:

$$\frac{ny}{1} - \frac{n.\,\overset{2}{\overline{n+1}}|^{2}}{\underset{\smile}{3}|^{3}} \times \frac{)^{3}}{t^{2}} + \frac{n.\,\overset{2}{\overline{n+3}}|^{4}}{\underset{\smile}{5}|^{5}} \times \frac{)^{5}}{t^{4}} - \frac{n.\,\overset{2}{\overline{n+5}}|^{6}}{\underset{\smile}{7}|^{7}} \times \frac{)^{7}}{t^{6}} + \text{\&c.}$$

according to *Cor.* 1. *Lem.* 1. the Area of the Sector will appear in a Series, as is above determined.

But since the Number n is greater than 2, and the given Arch P N is less than a Semicircle, and consequently K L or y, the Sine of the Submultiple Arch P K, is less than the Semidiameter C P or t; it may thence be easily proved, that the Series will approximate to the just Quantity of the Area, *ad libitum.*

Corol. 1. Hence, if the Number n be taken equal to $\sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$, the Sector N S P will be $= \frac{1}{2} npy + \frac{n^{3}t - n.\,\overline{nn-1}.\,p}{12tt} y^{3}$

$$+ **** + \frac{n^{3}}{1120\,t^{5}})^{7} + \text{\&c.}$$

For the Numerator of the Coefficient of the third Term in the Series, that determines the Area H, namely, $9t - \overset{2}{\overline{n+3}}|^{4} \times f$ is equal to $9t - \overline{nn-1}.\,\overline{nn-9}.\,f$, which according to the above Deter-

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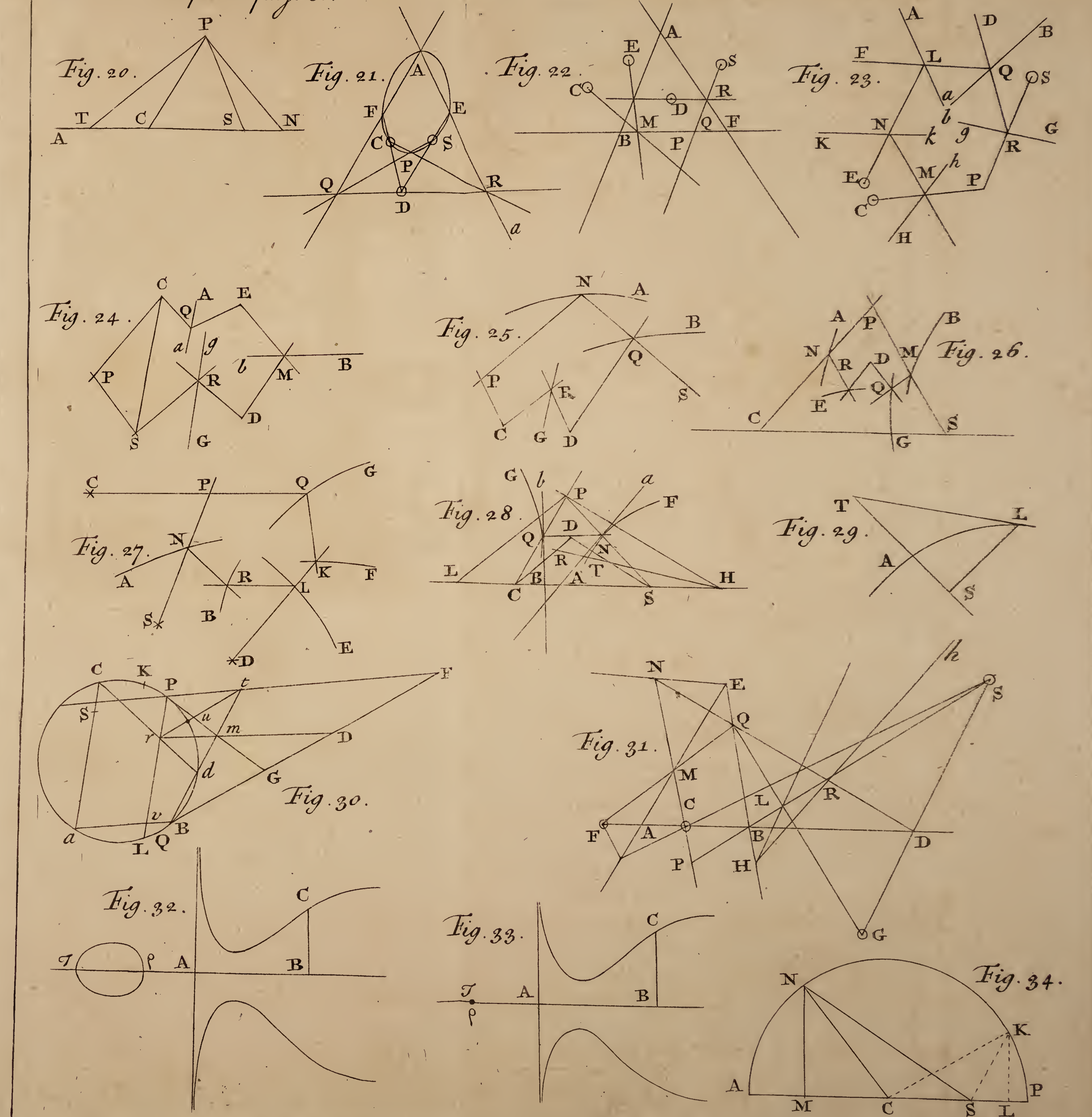

Determination of the Number n, will become nothing; wherefore, if for $t - p$ be put f in the second Term, and the Value of n be substituted for n in the Third and Fourth, the Series for the Area will appear upon Reduction to be as is here laid down.

Hence the Area of the Sector N S P may be always defined nearly by the Terms of a Cubic Equation. **Corol. 2.**

For the Number n, as constructed in the former *Corollary*, is always greater than the square Root of 10, and consequently $\frac{y}{t}$ is always less than the Sine of one third Part of the given Arch; so that the fourth Term $\frac{n^3}{1120t^5}y^7$, with the Sum of all the following Terms of the Series, can never be more than a small Part of the whole Sector.

If R stand for 57,2957795, &c. Degrees, (or the Number of Degrees contained in an Angle subtended by an Arch of the same Length with the Radius of the Circle) and M be the Number of Degrees in an Angle which is to 4 right Angles, as the Area N S P to the Area of the whole Circle; then will M be $= \frac{np}{t} \times \frac{Ry}{t}$ $+ \frac{n^3 t - n . \overline{nn - 1} . p}{6t} \times \frac{Ry^3}{t^3}$, nearly. **Corol. 3.**

For $\frac{M}{R} \times \frac{tt}{2}$ will appear by the Construction to be equal to the Sector N S P.

CASE II. If S P be greater than C P, then take an Area H equal to the Sum of the Terms in the following Series: $\frac{py}{1} + \frac{\overline{t - \overline{n^2+1}}\,\rceil^{2} \times f}{3|^3} \times \frac{y^3}{t^2} + \frac{\overline{9t + \overline{n^2+3}}\,\rceil^{4} \times f}{5|^5} \times \frac{y^5}{t^4} + \frac{\overline{9 \times 25t - \overline{n^2+5}}\,(^{6} \times f}{7|^7} \times \frac{y^7}{t^6} +$ &c. and the Area $\frac{1}{2} n \times H$, will be the Sector, as before.

For the Point S being on the contrary Side of the Centre to what it was before, it will easily appear, that the Change of $+f$ into $-f$, must reduce one Case to the other, without any other Proof.

Hence, if the Number n be taken equal to $\sqrt{\frac{t+f}{f}}$ or in this Case $\sqrt{\frac{p}{f}}$, then the Series for the Sector will want the second Term, as in the former it wanted the Third. **Corollary**

The

Definition. The Angle called by *Kepler* the *Anomalia Eccentri*, is a fictitious Angle in the Elliptic Orbit of a Planet, being analogous to the Area defcribed by a Line from the Centre of the Orbit, and revolving with the Planet from the Line of *Apfides*; in like manner as the *Mean Anomaly* is a fictitious Angle, analogous to the Area defcribed by a Line from the Focus.

Otherwife, if C be the Centre, S the Focus of an Elliptic Orbit defcribed on the tranfverfe Axis A P, and the Area N S P in the Circle be taken in Proportion to the whole, as the Area defcribed in the Ellipfis about the Focus, to the whole: Then is the Arch of the Circle P N, or the Angle N C P, that which *Kepler* calls the *Anomalia Eccentri*.

This Angle may be meafured either from the *Aphelion*, or from the *Perihelion*; in the following Propofition it is fuppofed to be taken from the *Perihelion*.

Prop. II. *The mean Anomaly of a Comet or Planet revolving in a given Elliptic Orbit being given; to find the* ANOMALIA ECCENTRI.

The Solution of this Problem requires two different Rules; the firft and principal one ferves to make a Beginning for a further Approximation, and the other is for the Progreffion in approximating nearer and nearer *ad libitum*.

I. *The Rule for the firft Affumption:* Let t, f, and p, ftand as before, for the Semi-tranfverfe Axis of the Ellipfis, the Semi-diftance of the Foci, and the *Perihelian* Diftance; then taking the Number n equal to $\sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$; let T ftand for $\frac{2t}{nnt - nn - 1 . p}$; and P for $\frac{2p}{nnt - nn - 1 . p}$ (or $\frac{p}{t}T$); which conftant Numbers, being once computed for the given Orbit, will ferve to find the Angle required nearly by the following Rule.

Let M be the Number of Degrees in the Angle of mean *Anomaly* to the given Time, reckoned from or to the *Perihelion*; and fuppofing R, as before, to ftand for 57,2957, &c. Degrees; take the Number $N = \sqrt[3]{\frac{3T}{nR}}M$, and let A be the Angle whofe Sine is $N\sqrt[3]{\frac{1}{2} + \sqrt{\frac{1}{4} + \frac{P^3}{N^6}}} + N\sqrt[3]{\frac{1}{2} - \sqrt{\frac{1}{4} + \frac{P^3}{N^6}}}$; then the Multiple Angle $n \times A$ will be nearly equal to the *Anomalia Eccentri*.

The Truth of which will appear from the Refolution of the Cubic Equation in the laft *Corollary* to the preceding Propofition.

If

If the Quadruple of the Quantity $\frac{P^3}{N^6}$ be many times greater or many times lefs than Unity; or, which amounts to the fame, if the mean *Anomaly* M, be many times lefs, or many times greater, than the Angle denoted by the given Quantity $\frac{2np}{3t} R\sqrt{P}$ (one or the other of which two Cafes moft frequently happens in Orbits of very large Eccentricity) then the Theorem will be reduced to a fimpler Form near enough for Ufe. *Corol.* 1.

If M be many times lefs than $\frac{2np}{3t} R\sqrt{P}$, then the Angle A may be taken for that whofe Sine is $\frac{t \times M}{np \times R}$. *Cafe* I.

If M be many times greater than $\frac{2np}{3t} R\sqrt{P}$, then let A be the Angle whofe Sine is $N - \frac{P}{N}$; and the Multiple Angle $n \times A$, according to it's Cafe, will be nearly equal to the Angle required. *Cafe* II.

In Orbits of very large Eccentricity, the *Perihelian* Diftance p is many times lefs than the Semi-diftance of the Foci f, and the Number $n = \sqrt{5 + \sqrt{25 + \frac{9p}{f}}}$; is always nearly equal to $\sqrt{10}$ or to the Integer 3, either of which may be ufed for it without any material Error in the Orbits of Comets. *Corol.* 2.

II. *The Rule for a further Correction* ad libitum.

Let M be the given mean *Anomaly*, t the Semi-tranfverfe Axis, as before; and let B be equal to or nearly equal to the Multiple Angle $n \times A$ before found, then if μ be the mean *Anomaly*, and x the Planet's Diftance from the Sun, computed to the *Anomalia Eccentri* B; the Angle $\overset{\prime}{B}$ taken equal to $\mathrm{B} + \frac{t}{x} \times \overline{\mathrm{M} - \mu}$, will approach nearer to the true Value of the Angle fought; and by Repetitions of the fame Operation, the Approximation may be carried on nearer and nearer, *ad libitum*.

This laft Rule being obvious, the Explication of it may be omitted at prefent.

In this Solution, where the Motion is reckoned from the *Perihelion*, the Rule is univerfal, and under no Limitation, but had the Motion been taken from the *Aphelion*, the Problem muft have been divided Scholium.

M 2 into

into two Cafes: One is, when the Eccentricity is lefs than $\frac{9}{16}$; the other is, when it is not lefs, but is either equal to, or more than in that Proportion.

If the Eccentricity be not lefs than $\frac{9}{16}$, then the fame Rule will hold, as before, only putting the *Aphelian* Diftance, fuppofe (a) inftead of the *Perihelian* Diftance (p), and fubftituting $-f$ for $+f$ in the Rule for the Number n.

If the Eccentricity be lefs than $\frac{9}{16}$, then take the Number n equal to $\sqrt{\frac{a}{f}}$, and $\frac{t}{na} \times \frac{M}{R}$ will be nearly equal to the Sine of the Submultiple Part of the *Anomalia Eccentri* denominated by the Number n, as before.

It is needlefs to obferve, that the like Rules would obtain in Hyperbolic Orbits, *mutatis mutandis.* But that which perhaps may not appear unworthy of being remarked, concerning this fort of Solution from the Cubic Root, is, that although the Rule be altogether impoffible, upon a total Change of the Figure of the Orbit either into a Circle, or into a Parabola; yet it will operate fo much better, and ftand in need of lefs Correction, according as the Figure advances nearer in it's Change towards either of thofe two Forms.

That the Ufe of the Method may better appear, it may not be amifs to add a few Examples.

I have given two for the Orbits of Planets, one the moft, and the other the leaft Eccentric; but which are more to fhew the Extent of the Rule, than to recommend the Ufe of it in fuch Cafes; for there are many other much better and more expeditious Methods in Orbits of fmall Eccentricity. The other two Examples are adapted to the Orbits of two Comets, whofe Periods have been already difcovered by Dr *Halley*; the one is to fhew the Ufe of one of the Rules in the firft *Corollary*, and the other is to explain the Ufe of the other Rule.

EXAMPLE I. *For the Orbit of* Mercury.

If an Unit be put for the Semi-tranfverfe Axis (t), the Eccentricity 0,20589 will become (f), and the *Perihelian* Diftance (p) will be 0,79411; wherefore by means of the Number R given as before, the conftant Numbers for this Orbit will appear to be, $n = 3{,}56755$, $T = 0{,}5857271$, $P = \frac{p}{t} T = 0{,}4651319$, and hence $\frac{3T}{n \times R} = 0{,}0085965$.

Example.

Suppofe M the mean *Anomaly* from the *Perihelion* to be 120°. 00′ 00″, to which it is required to find the *Anomalia Eccentri.*

Here, fince the mean *Anomaly* M is not many times more than the limiting Angle $\frac{2np}{3t} R \sqrt{P}$, (which in this Orbit is about 74 Degrees) Recourfe muft be had to the general Rule in the Propofition.

The

The Number N then, which is $\sqrt[3]{\frac{3\,T}{n\,R}}\,M$ will be $= 1,0104195$;

which found gives $N\sqrt[3]{\frac{1}{2}+\sqrt{\frac{1}{4}+\frac{P^3}{N^6}}} = 1,0389090$; and alſo

$N\sqrt[3]{\frac{1}{2}-\sqrt{\frac{1}{4}+\frac{P^3}{N^6}}} = -0,4477126$. Wherefore the Sum of

both (under their proper Signs) *viz.* 0,5911964 will be the Sine whoſe Arch 36°,24195 is the Angle A; the Multiple whereof $n \times A =$ 129°,295503, will be the Angle to be firſt aſſumed for the *Anomalia Eccentri.*

For a further Correction; this Angle, now called B, whoſe Sine is ſuppoſe y, and it's Coſine z, gives, by a known Rule, $t + \frac{f}{t}\,z =$ 1,1304 for x the Planet's Diſtance from the Sun; and by another known Rule $B - \frac{fR}{t\,t}\,y = 120°,16568$ for μ the mean *Anomaly* to the *Anomalia Eccentri B.* Wherefore the correct Angle $B = B + \frac{t}{x} \times \overline{M - \mu}$ will be $129°,14846 = 129°.\ 08'.\ 54'',5$, erring, as will appear from a further Correction, about $\frac{1}{10}$ of a Second.

This Angle being thus determined, will give by the common Methods 137°. 48′. 33″ ½, for the true *Anomaly* or Angle at the Sun: The Sine of the true *Anomaly* being in Proportion to the Sine of the *Anomalia Eccentri*, as the Semi-conjugate Axis to the Planet's Diſtance from the Sun. So that the Equation of the Center in this Example is 17°. 48′. 33″ ½.

Suppoſing, as before, the mean Diſtance t to be Unity, and the Eccentricity f to be 0,0069855; the conſtant Numbers for this Orbit will be, $p = 0,9930115$; $n = 6,4116$; $T = 1,562134$; $P =$ 0,1551217; $\frac{3\,T}{n\,R} = 0,0127571$; and the limiting Angle $\frac{2\,n\,p}{3\,t}$ $R\sqrt{P}$, will appear to be about 303 Degrees. EXAMPLE II. *For the Orbit of* Venus.

Let M be 120°. 00′. 00″, as in the former Example. Then, ſince the mean *Anomaly* is, in this Caſe, not many times leſs than the limiting Angle, the general Rule muſt be uſed as before; according to which the Number N will appear to be 1,152585; the Sine of A will be 0,3217917; the Angle A, 18°,77132; and the Multiple $n \times A$, or Angle B; for the firſt Aſſumption of the *Anomalia Eccentri* will be 120°,35416. *Example.*

This Angle B will give, by the Method before explained, the Angle $B = 120°,34555$, or $120°.\ 21'.\ 44''$ *ferè*, for the *Anomalia Eccentri* correct; the Error of which will appear, upon Examination, to be but a small Part of a Second.

In this Example the true *Anomaly* is $120°.\ 41'\ 25'',1$; and consequently the Equation of the Center no more than $41'.\ 25'',1$.

EXAMPLE III. *For the Orbit of the* Comet *of* 1682.

To know the mean *Anomaly* of this Comet to any given Time, it is to be premised, that it was at the *Perihelion* in the Year 1682, on the 4th Day of *September*, at 21 Ho. 22 Min. equated Time to the Meridian of *Greenwich*, and makes it's Revolution about the Sun, as Dr *Halley* has discovered, in $75\frac{1}{2}$ Years.

The *Perihelian* Distance p is, according to his Determination, 0,0326085 Parts of the mean Distance t. So that the constant Numbers for the Orbit will be, $n = 3,1676061$; $T = 0,2054272$; $P = 0,00669867$; and the limiting Angle $\frac{2\,np}{3\,t}\ R\ \surd\ P$ will be about 19 Minutes or $\frac{1}{3}$ of a Degree.

In the Orbits of Comets, the Rule for the first Assumption of the *Anomalia Eccentri* is generally sufficient without Correction.

Thus, suppose the mean *Anomaly* M to be 0,072706, (as it was at the Time of an Observation made at *Greenwich* on the 30th of *August* 1682, at $7^h\ 42'$. Æq. T.) then the general Rule (which must be here used, since the Angle of mean *Anomaly* is not above 4 or 5 Times less than the limiting Angle) will give $n \times A$ or $B = 2°.\ 12'.\ 48'',7$, erring about $\frac{7}{10}$ of a Second from the true *Anomalia Eccentri*.

But in these Orbits the Rules in the first *Corollary* to the second Proposition most frequently take Place, especially the last; and the Calculation may also be further abbreviated, by putting the square Root of 10, or the Integer 3, for the Number n.

Example.

Suppose the mean *Anomaly* to be $0°,006522$, or $23'',4792$: Here, since M is 50 Times less than the limiting Angle, the Rule in the first Case of the first *Corollary* may be used; that is, to take the Sine of the Angle $A = \frac{t \times M}{n\,p \times R}$.

Wherefore, if the Number 3 be put for n, the Sine of A, which is $\frac{t\,M}{3\,p\,R}$, will be $= 0,00116367$; and consequently the Angle A will be $4'.\ 00''\ 011$; and the multiple Angle $n \times A$ to be assumed for the *Anomalia Eccentri* will be $12'.\ 00'',033$, the Error of which will be found to be about $\frac{1}{30}$ of a Second.

EXAMPLE IV. *For the Orbit*

This Comet, according to Dr *Halley*, performs it's Period in 575 Years; and was in it's *Perihelion* on the 7th of *December* 1680, at

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23 Hours $09'$ Æq. T. at *London*; the *Perhelian* Diſtance p is 0,000089301, in Parts of the mean Diſtance t: Wherefore ſuppoſing the Number n to be $\sqrt{10}$, the conſtant Numbers for the Orbit will be $T = 0{,}2000161$; $P = 0{,}000017862$, and the limiting Angle $\frac{2np}{3t} R\sqrt{P}$ will be about $\frac{1}{6}$ of a Second. *of the great Comet of the Year* 1680.

Suppoſe the mean *Anomaly* to be $3'$, $31''{,}4478$ or $0^{\circ}{,}05873541$, (as it was at the Time of the firſt Obſervation made on it in *Saxony*, on *November* the 3d, at $16^{h.}\ 47'$ Æq. T. at *London*.) here, ſince the mean *Anomaly* is many times greater than $\frac{1}{6}$ of a Second, the Rule in the ſecond Caſe of the firſt *Corollary* may be uſed; that is, by taking the Sine of $A = N - \frac{P}{N}$. *Example.*

But the Number N or $\sqrt[3]{\frac{3T}{nR}}$ is $= 0{,}05794134$; and $\frac{P}{N}$ will be $= 0{,}0030827$; wherefore $(N - \frac{P}{N} =)$ $0{,}05763307$, will be the Sine whoſe Arch $3^{\circ}{,}30397$ is the Angle A; and the multiple Angle $n \times A = 10^{\circ}.\ 26'.\ 53''{,}05$, will be the Angle to be firſt aſſumed for the *Anomalia Eccentri*; the Error of which will be found to be leſs than a Second.

The true *Anomaly*, computed from this Angle according to the Rule in the Example for *Mercury*, will appear to be $171^{\circ}.\ 38'.\ 24''$. from the *Perihelion*.

By theſe Examples it appears, that the Solution is univerſal in all Reſpects; for the two firſt, compared with the two laſt, ſerve to ſhew that it is not confined to any particular Parts of the Orbit, but extends to all Degrees of mean *Anomaly*: And by comparing the ſecond with the laſt, it ſufficiently appears to be univerſal with reſpect to the ſeveral Degrees of Eccentricity; ſince in one the Equation of the Center for the Reduction of the Mean to the true Motion is not ſo much as the $\frac{1}{170}$th Part of the whole; whereas in the other it amounts to almoſt 3000 times as much as the mean Motion itſelf.

Upon reviewing the Reflections on the Quadrature of the Circle in Page 77, I believe it may be neceſſary for me to prevent any Miſtake that may ariſe from the different Opinions that obtain about the Nature of Mathematical Quantity, to explain myſelf a little upon that Head; as alſo to add a few Words to ſhew how the Method of Quadrature by limiting Polygons, takes Place in other Figures as well as the Circle. Poſtſcript.

I take then a Mathematical Quantity, and that for which any Symbol is put, to be nothing elſe but Number with Regard to ſome Meaſure which is conſidered as one. For we cannot know preciſely and deter-

determinately, that is, mathematically, how much any thing is, but by means of Number. The Notion of continued Quantity, without regard to any Meaſure, is indiſtinct and confuſed; and although ſome Species of ſuch Quantity, conſidered phyſically, may be deſcribed by Motion, as Lines by Points, and Surfaces by Lines, and ſo on; yet the Magnitudes or Mathematical Quantities are not made by that Motion, but by numbering according to a Meaſure.

Accordingly, all the ſeveral Notations that are found neceſſary to expreſs the Formations of Quantities, do refer to ſome Office or Property of Number or Meaſure; but none can be interpreted to ſignify continued Quantity as ſuch.

Thus ſome Notations are found requiſite to expreſs Number in it's ordinal Capacity or the *Numerus Numerans*, as when one follows or precedes another, in the firſt, ſecond, or third Place from that upon which it depends; as the Quantities $\grave{x}$, $\grave{\grave{x}}$, x, $\acute{x}$, $\acute{\acute{x}}$, referring to the principal one x.

So, in many Caſes, a Notation is found neceſſary to be given to a Meaſure as a Meaſure; as for Inſtance, Sir *I. Newton*'s Symbol for a Fluxion $\dot{x}$; for this ſtands for a Meaſure of ſome Kind, and accordingly he uſually puts an Unit for it, if it be the principal one upon which the reſt depend.

So ſome Notations are expreſsly to ſhew a Number in the form of it's Compoſition, as the Index to the Geometrical Power x^n denoting the Number of equal Factors which go to the Compoſition of it, or what is analogous to ſuch.

But that there is no Symbol or Notation but what refers to diſcreet Quantity, is manifeſt from the Operations, which are all Arithmetical.

And hence it is, there are ſo many Species of Mathematical Quantity as there are Forms of compoſite Numbers, or Ways in the Compoſition of them; among which there are two more eminent for their Simplicity and Univerſality than the reſt: One is the *Geometrical Power* formed from a conſtant Root; and the other, though well known, yet wanting a Name as well as a Notation, may be called the *Arithmetical Power*; or the Power of a Root uniformly increaſing or diminiſhing, and is that whoſe Notation is deſigned in Page 78: The one is only for the Form of the Quantity itſelf, the other is for the Conſtitution of it from it's Elements.

Now from the Properties of either of theſe it would be eaſy to ſhew how the Quadratures of ſimple Figures are deducible from the Areas of their limiting Polygons. I ſhall juſt point out the Method from the Arithmetical Power, as being the ſhorteſt and readieſt at Hand.

Let z, $\acute{z}$, $\acute{\acute{z}}$, &c. or z, $\grave{z}$, $\grave{\grave{z}}$, &c. be Quantities in Arithmetical Progreſſion, diminiſhing or increaſing by the common Difference

rence $\dot{z}$, and let, as before explained, $\overset{\dot{z}}{\frown}\!\!\!z^{(m}$ signify the Arithmetical Power of z, denominated by the potential Index m, namely, $z \times z' \times z''$, &c. whose first Root is z and last $z - m - 1 \times \dot{z}$; which being supposed, the Element of the Arithmetical Power will be $m \dot{z} \times \overset{\dot{z}}{z'}{}^{(m-1}$ that is, the Product made from the Multiplication of the two Indices, and the next inferior Power of the next Root in Order. For the first Arithmetical Power $\overset{\dot{z}}{z}{}^{(m}$ is $= z.\ \overset{\dot{z}}{z'}{}^{(m-1}$, and the next $\overset{\dot{z}}{z'}{}^{(m}$ is $= \overset{\dot{z}}{z'}{}^{(m-1} \times \overline{x - m\dot{z}}$, wherefore the Difference will be as is explained.

And consequently, since the Sum of these Elements or Differences, taken in order from the first to the last, do make up the Quantity according to it's *termini*; hence, if z be the Abscis of a curvilinear Figure whose Ordinate y is equal to $m z^{m-n}$, a Demonstration might easily be made that [the Form of the Quantity for] the Area will be z^{m}; that is, the same Multiple of the next superior Power of z divided by the Index of that Power.

For since the Arithmetical Powers do both unite and become the same with the Geometrical Power, when the differential Index $\dot{z}$ is supposed to be nothing; the Magnitude of the Geometrical Figure will be implied from the Magnitudes of the two Polygons made up of Rectangles, one from the increasing Arithmetical Power, the other from the diminishing, although it be true, that the Elements of the Polygons cannot be summed up, when $\dot{z}$, the Measure of the Abscis z, is supposed to be nothing.

In like manner, in any other Case where z and z' are two Abscisses whose Difference as a Measure is $\dot{z}$; and y, y' the two Ordinates; the Magnitude of the Figure will be implied by the Magnitudes of the two Polygons which are made from the Sum of the inscribing and circumscribing Elements $\dot{z} y$ and $\dot{z} y'$, although the Figure itself is not to be resolved into any such primogenial rectangular Elements.

And thus, I think, the Symbol $\dot{z}$, considered as a component Part of the Rectangle $\dot{z} y$, may bear a plain Interpretation; *viz.* that it is the Measure according to which the Quantity z is measured; nor can I see that any other Interpretation need to be put upon a Symbol, which, like a Measure, is used only to make other things known, but is of itself for nothing but a Mark.

 And

And what is ſaid of the Elements of the firſt Reſolution, is eaſily applied to thoſe of a ſecond or third, and ſo on; the laſt may always be conſidered as the Meaſure of the former and indiviſible, although, in reſpect of the following, it be taken as the Part according to which the Meaſure was made, and therefore diviſible.

An Inquiry concerning the Figure of ſuch Planets as revolve about an Axis, ſuppoſing the Denſity continually to vary, from the Centre towards the Surface; by Mr Alexis Clairaut, *F. R. S. and Member of the Royal Acad. of Sciences at* Paris. *Tranſlated from the* French *by the Rev.* John Colſon, *Lucaſ. Prof. Math.* Cantab. *and F. R S.* N°. 449. p. 277. *Aug.* &c. 1738.

IX. Notwithſtanding that Part of Sir *I. Newton*'s *Mathematical Principles of Natural Philoſophy*, where he treats of the Figure of the Earth is delivered with the uſual Skill and Accuracy of that great Author; yet I thought ſomething farther might be done in this Matter, and that new Inquiries may be propoſed, which are of no ſmall Importance, and which poſſibly he overlooked, through the Abundance of thoſe fine Diſcoveries he was in Purſuit of.

What at firſt ſeemed to me worth examining, when I applyed myſelf to this Subject, was to know why Sir *Iſaac* aſſumed the Conical Ellipſis for the Figure of the Earth, when he was to determine it's Axis? For he does not acquaint us why he did it, neither can we perceive how he had ſatisfied himſelf in this Particular: And unleſs we know this, I think we cannot entirely acquieſce in his Determinations of the Axes of the Planets. It ſeems as if he might have taken any other oval Curve, as well as the Conical Ellipſis of *Apollonius*, and then he would have come to other Concluſions about thoſe Axes.

I began then with convincing myſelf by Calculation, that the Meridian of the Earth, and of the other Planets, is a Curve very nearly approaching to an Ellipſis; ſo that no ſenſible Error could enſue by ſuppoſing it really ſuch. I communicated my Demonſtration of this to the *Royal Society*, at the Beginning of the laſt Year; and I have ſince been informed, that Mr *Stirling* *, had inſerted a Diſcourſe in the *Philoſophical Tranſactions*, wherein he had found the ſame thing before me, but without giving his Demonſtration. When I ſent that Paper to *London*, I was in *Lapland*, within the frigid *Zone*, where I could have no Recourſe to Mr *Stirling*'s Diſcourſe, ſo that I could not take any Notice of it.

The Elliptical Form of the Meridian being once proved, I no longer found any thing in Sir *I. Newton*, about the Figure of the Earth, which could create any new Difficulty; and I ſhould have thought this Queſtion ſufficiently diſcuſſed, if the Obſervations made under the Arctick Circle had not prevailed on us to believe, that the Shape of the Earth was ſtill flatter than that of Sir *Iſaac*'s Spheriod; and if he himſelf had not pointed at the Cauſes, which might make *Jupiter* not quite ſo flat, as by his Theory, and the Earth ſomething more.

As to *Jupiter*, he ſays †, that it's Equator conſiſts of denſer Parts than the reſt of it's Body, becauſe it's Moiſture is more dried up by the Heat of the Sun. But as to the Earth, he ſuſpects it's Flatneſs to be a ſmall matter greater than what ariſes by his Calculation. He

* See Chap. VII. of this Volume. † Princip. Math. Edit. 3. p. 416.

inſinuates

inſinuates, that it may poſſibly be more denſe towards the Center than at the Superficies *. I am ſomething ſurprized that Sir *Iſaac* ſhould imagine, that the Sun's Heat can be ſo great at *Jupiter*'s Equator, when it has no ſuch Effect at that of the Earth; and that he does not aſcribe each to a like Cauſe, by ſuppoſing alſo, that *Jupiter* may be of a different Denſity at the Center from that at the Superficies.

But whatever Reaſon he might have for introducing two different Cauſes, I give the Preference to the Hypotheſis which ſuppoſes unequal Denſities at the Center and at the Circumference. I have inquired, by the Aſſiſtance of this Theory, what would be the Figure of the Earth, and of the other Planets which revolve about an Axe, on Suppoſition that they are compoſed of ſimilar *Strata*, or Layers, at the Surface; but that their variable Denſity, from the Center towards the Circumference, may be expounded by any Algebraical Equation whatſoever.

And though my Hypotheſis ſhould not be conformable to the Laws of Nature, or even though it ſhould be of no real Uſe (which would be the Caſe, if the Obſervations made by the Mathematicians now in *Peru*, compared with ours in the North, ſhould require that Proportion of the Axes, which is derived from Sir *Iſaac*'s Spheroid;) I thought however that Geometricians would be pleaſed with the Speculations contained in this Paper, as being, if not uſeful, yet curious Problems at leaſt.

PART I. *In which are found the Laws of Attraction, which are exerted upon Bodies at a Diſtance, by a Spheroid compoſed of Orbs of different Degrees of Denſity.*

To find the Attraction which a homogeneous Spheroid B N E b e, *differing but very little from a Sphere, exerts upon a Corpuſcle placed at* A *in the Axis of Revolution.*

Prob. I. Fig. 35.

I. We may conceive the Space B N E b D M B, included between the Spheroid and the Sphere, to be divided into an infinite Number of Sections perpendicular to the Axe A C b. Suppoſing then that every one of the Particles, which are contained in one of theſe Elements or Moments N n m M, exerts the ſame Quantity of Attraction upon the Body at A, which may be ſuppoſed becauſe of the Smallneſs of N M; we ſhall have $c\,\alpha \times PM^2 \times Pp \times \frac{AP}{AM^3}$ for the Attraction of any one of thoſe Elements; putting c for the Ratio of the Circumference to the Radius, and α for the given Ratio of M N to P M, that is, of D E to C D.

Now if we make C A $= e$, C B $= r$, A M $= z$; and for P M, A P, P p, if we ſubſtitute their Values expreſſed by z, and then ſeek the Fluent of the foregoing Quantity; we ſhall have $\frac{4\,c\,\alpha\,r^3}{3\,e\,e}$

* See Sect. xxiv.

$-\frac{4c\alpha r^5}{5e^4}$ for the Value of the whole Attraction of the Solid generated by the Revolution of B D b E B: To which if we add $\frac{2r^3c}{3ee}$ the Attraction of the Sphere, we ſhall have $\frac{2r^3c}{3ee}+\frac{4cr^3\alpha}{3ee}$ $-\frac{4c\alpha r^5}{5e^4}$ for the required Attraction of the Spheroid upon the Corpuſcle A.

Prob. II. Fig. 36. *Suppoſing now the Spheroid* B e b e, *to be no longer of a homogeneous Matter, but to be compoſed of an infinite Number of Elliptical* Strata, *all ſimilar to* B E b, *the Denſities of which are repreſented by the Ordinates* K T *of any Curve whatever* V T, *of which we have the Equation between* C K *and* K T; *the Attraction is required which this Spheroid exerts upon a Corpuſcle placed at the Pole* B.

II. Making B C $= e$, C K $= r$, by the foregoing Propoſition, we ſhould have $\frac{2r^3c}{3ee}+\frac{4cr^3\alpha}{3ee}-\frac{4c\alpha r^5}{5e^4}$ for the Attraction of the Spheroid K L K, if it conſiſted of homogeneous Matter; and the Fluxion of this Quantity $\frac{2rrc\dot{r}}{ee}+\frac{4c\alpha r^2\dot{r}}{ee}-\frac{4c\alpha r^4\dot{r}}{e^4}$ would be the Element or Moment of the Orb K L K k l k. But becauſe the Denſity is variable, we muſt multiply this Value of the Attraction of the Orb by K T, and the Fluent of this Quantity will be the Value of the Attraction of the Spheroid K L K.

As to the Value of K T, which expreſſes the Denſity of the Stratum or Bed K L K k l k, we ſhall take only fr^p+gr^q, becauſe we ſhall ſee afterwards, that a Value more compounded, at fr^p+gr^q, $+hr^s+ir^t$, *&c.* which by the Property of Series may expreſs all Curves, would not produce any Variety in the Calculation.

Therefore multiplying the foregoing Equation by fr^p+gr^q, we ſhall have $\frac{2cf\times\overline{1+2\alpha}\times r^{3+p}}{ee\times\overline{3+p}}-\frac{4c\alpha fr^{5+p}}{e^4\times\overline{5+p}}$ $+\frac{2cg\times\overline{1+2\alpha}\times r^{3+q}}{ee\times\overline{3+q}}-\frac{4c\alpha gr^{5+q}}{e^4\times\overline{5+q}}$ for the Quantity of Attraction of the Spheroid K L K, exerted upon a Corpuſcle placed at B.

III. In this Value making $r = e$, we ſhall have $\frac{2cfe^{\overline{1+p}}}{3+p}$

$$+ \frac{8cfe^{\overline{1+p}}\alpha}{\overline{3+p}\times\overline{5+p}} + \frac{2cge^{\overline{1+q}}}{3+q} + \frac{8cge^{\overline{1+q}}\alpha}{\overline{3+q}\times\overline{5+q}}$$ which will expreſs the Force of Attraction of the Spheroid B E b, exerted upon a Corpuſcle placed at the Pole B.

A Corpuſcle being placed in any Point N *of the Surface of the foregoing Spheroid* B E *b e*, *I ſay it will undergo the ſame Attraction from this Spheroid, as if it were placed at the Pole* N *of a ſecond Spheroid revolving about the Axe* N O, *the ſecond Axe being the Radius of a Circle equal in Superficies to the Ellipſis* F G; *ſuppoſing this ſecond Spheroid* N G O F, *to be compoſed of the* Strata M *m q* Q, *whoſe Denſities are the ſame as thoſe of the* Strata K *k* L *l* K *k*, *of the firſt Spheroid.* THEOREM. Fig. 37.

IV. In the Diſcourſe which I communicated to the Royal Society *, being then at *Torneo*, printed in the *Philoſophical Tranſactions*, I have demonſtrated this Propoſition as to a homogeneous Spheroid; and the ſame Reaſoning will obtain in this Caſe alſo.

To find the Attraction which the Spheroid B *e b e exerts upon a Corpuſcle placed at any Point* N *of the Superficies.* PROB. III. Fig. 36.

V. We will make, as above, B C $= e$, C E $= e + e\alpha$, and alſo C N $= e + e\lambda$, and half the Conjugate Diameter of C N will be C G $= e + e\alpha - e\lambda$; whence the Radius of a Circle, equal in Superficies to the Ellipſis F G, will be a mean Proportional between C E and C G, that is to ſay, $e + e\alpha - \frac{1}{2}e\lambda$. Therefore the Spheroid B E b e exerts the ſame Attraction at N, as would be exerted at the Pole of a Spheroid N G O F, (*Fig.* 37.) of which the principal Axis would be N O $= 2e + 2e\lambda$; and the ſecond would be to the Principal as $1 + \alpha - \frac{3}{2}\lambda$ to 1.

Therefore in the Expreſſion of the Attraction at the Pole, (Art. III.) we muſt ſubſtitute $e + e\lambda$ inſtead of e, and $\alpha - \frac{3}{2}\lambda$ inſtead of α.

* See Chap. vii. of this Volume.

But

But if f and g must no longer be the same; for we may easily perceive by the foregoing *Theorem*, that the Density must be the same in this Speroid N G O F, at the Distance $r + r\lambda$ from the Center, as it is in the Spheroid B E b e at the Distance r. Therefore $f\left(\frac{e}{1+\lambda}\right)^p + g\left(\frac{e}{1+\lambda}\right)^q$ must be put instead of $fe^p + ge^q$.

Thus we shall have $\frac{2cfe^{\overline{1+p}}}{3+p} + \frac{\overline{2p-2}\,cf\lambda e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} +$

$\frac{8cf\alpha e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \frac{2cge^{\overline{1+q}}}{3+q} + \frac{\overline{2q-2}\,cg\lambda e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}}$

$+ \frac{8cg\alpha e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}}$ for the Attraction of the Spheroid B E b e at N.

VI. If we make $\lambda = \alpha$, the foregoing Expression will be reduced to this $\frac{2cfe^{\overline{1+p}}}{3+p} + \frac{2cfe^{\overline{1+p}}\alpha}{5+p} + \frac{2cge^{\overline{1+q}}}{3+q} +$

$\frac{2cge^{\overline{1+q}}\alpha}{5+q}$, which expresses the Attraction of the Equator.

VII. If we would have the Attraction at any Point M within the Spheroid, in the Expression of the Attraction at N, we must put r instead of e. The Proof of this is plain from the same Reasons that Sir *I. Newton* makes use of *, to shew that the Attraction of an elliptic Orb, at a Point within it, is none at all.

Prob. IV. Fig. 38. *Let* R Π r π *be a Circle whose Center is* Y; *'tis required to find the Attraction which this Circle exerts upon a Corpuscle at* N, *according to the Direction* H Y; *supposing the Point* H, *which answers perpendicularly below the Point* N, *to be at a very small Distance from the Point* Y.

VIII. Let there be drawn Π H π perpendicular to the Diameter R Y r, and let the Space R Π π be transferred to π Π Z. Then the Space π Z Π r will be the only Part of R Π r π, which will attract the Body N according to H Y.

* *Princip. Math. Lib.* I. *Prop.* 91. *Corol.* 3.

To

To find the Attraction of this little Space, we will suppose it to be divided into the Elements $T\,t\,s\,S$, the Attractions of which, according to HY, will be $\frac{T\,t\,s\,S \times QT}{NT^3}$, or $\frac{2\,HY \times Q\,q \times QT}{NT^3}$, the Fluent of which $\frac{2\,HY \times HQTZ}{NT^3}$ is the Attraction of $TZ\,r\,S$, according to HY. In which if we put $\Pi\,\pi$ for HQ, we shall have $\frac{\Pi H \pi R \times 2\,HY}{NT^3}$, or $\frac{\frac{1}{2}HY \times \Pi H^2 \times c}{NT^3}$, for the Attraction required.

IX. It is easy to perceive, that if, instead of a Circle, the Curve $R\,\Pi\,r$ were an Ellipsis, or any other Curve whose Axes were but very little different from one another, the foregoing Solution would be still the same.

To find the Attraction which an Elliptical Spheroid $KL\,k$ *exerts upon a Corpuscle placed without it's Surface at* N, *according to the Direction* CX *perpendicular to* CN. PROB. V. Fig. 39.

X. To perform this, we will begin by drawing the Diameter $C\,\mu\,\nu$, which bisects the Lines $R\,r$ perpendicular to CN; and the Ratio of CH to HY shall be called n. Then esteeming the Ellipsis $R\,r$ as a Circle, (see the foregoing Article) we shall have by the Problem aforegoing $\frac{\frac{1}{2}n\,c \times RH^2 \times CH}{NR^3}$ for it's Attraction, according to HY; which being multiplyed by the Fluxion of MH, the Fluent of this will be the Attraction of the Segment of the Spheroid $RM\,r$.

This Calculation being made, and $N\,m$ being substituted for NR, we shall have $\frac{2\,n\,c\,r^5}{5\,e^4}$ for the Attraction of the Spheroid in N, according to the Direction CX.

To find the Attraction of a Corpuscle N, *according to* CX, *towards an Ellipsoid* $BNE\,b\,e$, *composed of Strata, the Densities of which are defined by the Equation* $D = f\,r^p + g\,r^q$. PROB. VI.

XI. Take the Fluxion of the Quantity $\frac{2\,c\,n\,r^5}{5\,e^4}$, which expresses the Attraction of the homogeneous Ellipsoid $KL\,k$, and you will have $\frac{2\,c\,n\,r^4\,\dot{r}}{e^4}$ for the Attraction of an infinitely little elliptic Orb; which

being multiplied by the Denſity D, gives $\frac{2cnfr^{4+p}\dot{r}}{e^{4}} + \frac{2cgnr^{4+q}\dot{r}}{c^{4}}$

the Fluent of which $\frac{2cfnr^{5+p}}{5+p\times e^{4}} + \frac{2cgnr^{5+q}}{5+q\times e^{4}}$, is the Attraction of the Spheroid K L k, according to C X. Therefore the total Attraction of the Spheroid B N E b e upon the Corpuſcle N, according to the Direction C X, will be $\frac{2cfne^{1+p}}{5+p} + \frac{2cgne^{1+q}}{5+q}$.

Now if we have regard to the Smallneſs of the Line N ν, and obſerve how little the Angle ν N C will differ from a right one, we may perceive that the Diameter C N contains the ſame Angle with the perpendicular N X in N, as the Diameter C N with the perpendicular at ν; that is to ſay, that the Angle N C ν is the ſame as the Angle C N X; ſo that inſtead of n we may take $\frac{CX}{CN}$. Wherefore the foregoing Expreſſion of the Attraction of the Ellipſoid B E b e, acting according to the Direction C X upon a Corpuſcle placed in N, will be $\frac{2cfe^{1+p}}{5+p} \times \frac{CX}{CN} + \frac{2cge^{1+q}}{5+q} \times \frac{CX}{CN}$.

Prob. VII. *To find the Direction of the Attraction of a Corpuſcle* N *towards the Ellipſoid.*

XII. by the ſecond Problem we ſhall find the Attraction of the Spheroid according to C N to be $\frac{2cfe^{1+p}}{3+p} + \frac{2cge^{1+q}}{3+q}$, by expunging what may be here expunged. Then by taking a fourth proportional to theſe three Quantities, the firſt of which is the Attraction according to C N, the ſecond is that according to C X, and the third is the right line C N; there will ariſe

$$\frac{\frac{fe^{1+p}}{5+p} + \frac{ge^{1+q}}{5+q}}{\frac{fe^{1+p}}{3+p} + \frac{ge^{1+q}}{3+q}} \times CX = CI.$$

Whence

Whence we ſhall have N I for the Direction required, of the Attraction of the Corpuſcle N.

XIII. If we ſuppoſe $p=q=0$, that is, if the Spheroid be homogeneous, we ſhall have $CI=\frac{3}{5}CX$; which agrees with what Mr *Stirling* has found, in that curious Diſſertation he has publiſhed in the *Philoſophical Tranſactions, ut ſupra.*

PART II. *The Uſe of the foregoing Problems, in finding the Figure of Spheroids, which revolve about an Axis.* Fig. 39.

XIV. Let us now ſuppoſe, that the foregoing Spheroid B N E b e, which is ſtill compoſed of Beds or *Strata* of different Denſities, revolves about it's Axis B b, and that it is now arrived at it's permanent State. It is plain that the Particles of the Fluid, which are upon it's Surface, muſt gravitate according to a Direction perpendicular to the Curvature B N E; for without this Condition there could be no *Æquilibrium.*

We ſhall now inquire, whether the Elliptic Figure we have aſcribed to our Spheroids can have this Property, and to produce this Effect what muſt be the Relation between the Time of Revolution of the Spheriod and the Difference of it's Axes.

Let us then put Φ for the centrifugal Force at the Equator, and the centrifugal Force at N will be $\frac{\phi \times PN}{CE}$, or $\frac{\phi \times Cx}{2CE \times \alpha}$, becauſe $2PN \times \alpha = Cx$.

By reſolving this centrifugal Force according to the Perpendicular to C N, we ſhall have $\frac{\phi \times CX}{2\alpha \times CE}$; which being added to $\frac{2cfe^{1+p}}{5+p} \times \frac{CX}{CN} + \frac{2cge^{1+q}}{5+q} \times \frac{CX}{CN}$, found by Prob. V. will give the whole Force of the Body N, according to the Direction C X, when the Spheroid is converted about it's Axis. But becauſe this Body, by virtue of the Attraction according to C N, and the Force according to C X, ought to have a perpendicular Tendency to the Superficies; we ſhall have this Analogy, C N. C X :: $\frac{2cfe^{1+p}}{3+p} + \frac{2cge^{1+q}}{3+q} \cdot \frac{\phi}{2\alpha} \times \frac{CX}{CE} + \frac{2cfe^{1+p}}{5+p} \times \frac{CX}{CN} + \frac{2cge^{1+q}}{5+q} \times \frac{CX}{CN}$. And hence, becauſe C N and C E may be aſſumed as the ſame on this Occaſion, it will be

$$\phi = \frac{8cfe^{1+p}\alpha}{3+p \times 5+p} + \frac{8cge^{1+q}\alpha}{3+q \times 5+q}.$$

 And

The Spheroid being supposed elliptical, Bodies will gravitate perpendicularly to it's Surface.

And as in this Value of the centrifugal Force, no Quantity enters but what will agree to any Point N; we may therefore conclude, that when our supposed elliptical Spheroid performs it's Rotation in a proper Time, so that the centrifugal Force at the Equator may be as before; then the centrifugal Force in any other Place N will be such as it ought to be, to cause Bodies to gravitate in a perpendicular Direction to the Surface.

The Expression for the Gravity at any Place on the Spheroid.

Fig. 40.

XV. If we now consider, that E D being taken for the centrifugal Force in E, then will M N express the centrifugal Force in N, and consequently M I will be such a Part of this Force as acts according to N C; we shall have $\frac{8cfe^{\overline{1+p}}\lambda}{\overline{3+p}\times\overline{5+p}} + \frac{8cge^{\overline{1+q}}\lambda}{\overline{3+q}\times\overline{5+q}}$ to be subtracted from the Attraction at N. Hence

$$\frac{2cfe^{\overline{1+p}}}{3+p} + \frac{\overline{2p-10}\,cf\lambda e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \frac{8cf\alpha e^{\overline{1+p}}}{\overline{3+q}\times\overline{5+q}} + \frac{2cge^{\overline{1+q}}}{3+p} + \frac{\overline{2q-10}\,cg\lambda e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}} + \frac{8cg\alpha e^{\overline{1+q}}}{\overline{8+q}\times\overline{5+q}}$$

will be the Gravity at N.

The Gravity at the Equator.

XVI. In this Value making $\lambda=\alpha$, we shall have

$$\frac{2cfe^{\overline{1+p}}}{3+p} + \frac{\overline{2p-2}\,cf\alpha e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \frac{2cge^{\overline{1+q}}}{3+q} + \frac{\overline{2q-2}\,cg\alpha e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}}$$

for the Gravity at the Equator.

XVII. If we subtract the Value of the Gravity in N from the Value of the Attraction or Gravity at the Pole, (Art. III.) we shall have $\frac{\overline{10-2p}\,cf\lambda e^{\overline{1+p}}}{\overline{3+p}\times\overline{5+p}} + \frac{\overline{10-2q}\,cg\lambda e^{\overline{1+q}}}{\overline{3+q}\times\overline{5+q}}$. But it is easy to perceive, that λ is proportional to the Square of the Sine of the Arc P M, or of the Complement of the Latitude. Whence we may therefore conclude, that the Diminution of the Gravity from the Pole to the Equator is proportional to the Square of the Cosine of the Latitude; or, which is the same thing, that *the Augmentation of Gravity from the Equator to the Pole is as the Square of the Sine of the Latitude*, as Sir *I. Newton* has demonstrated in his Hypothesis of a homogenous Spheroid.

XVIII. From the following Calculation it is easy to conclude, that Sir *Isaac*'s Theorem *, which is this, that *the Gravity in any Place within*

* *Prin. Math.* Lib. 3. Prop. 20.

is reciprocally as the Diſtance from the Centre, cannot obtain here. For we may ſee by the foregoing Expreſſion, that the Gravity in N cannot be to the Gravity in P as 1 to $1+\lambda$, except when $p=q=0$, which happens only in Sir *Iſaac*'s homogeneous Spheroid.

It was for want of conſidering, that this Theorem was demonſtrated by Sir *Iſaac* only in the Caſe of his homogeneous Spheroid, that ſeveral Geometricians have too haſtily concluded, this Theorem might be applied to determine the Ratio of the Earth's Axes, and the Lengths of the Pendulum obſerved in two Places of different Latitudes. Dr *Gregory* is one of thoſe who have fallen into this Miſtake *, And in the *Philoſ. Tranſact.* † it is concluded, from the Proportion of Gravity at *Jamaica* to that at *London*, that the Diameter of the Equator muſt exceed the Earth's Axis by $\frac{1}{190}$th Part, which Computation was founded on this 20th Prop. Lib. III. of Sir *Iſaac*'s *Principia*, which is true only of his Spheroid.

The Manner of finding the Axes of the Spheroid, the Variation of the Denſities of the Strata *being taken at pleaſure.*

XIX. Let us now ſuppoſe, that the centrifugal Force at the Equator is known by obſervation, as alſo within the Earth, *&c.* and that it is a certain Part $\frac{1}{m}$ of the Gravity; by Articles XIV, and XVI, we ſhall have this Equation:

$$\frac{2cfe^{1+p}}{3+p}+\frac{\overline{2p-2cf}\,e^{1+p}\alpha}{\overline{3+p}\times\overline{5+p}}+\frac{2cge^{1+q}}{3+q}+\frac{\overline{2q-2cg}\,e^{1+q}\alpha}{\overline{3+q}\times\overline{5+q}}=\frac{8cfme^{1+q}\alpha}{\overline{3+p}\times\overline{5+p}}+\frac{8cmge^{1+q}\alpha}{\overline{3+q}\times\overline{5+q}}.$$

From hence it will be eaſy to derive the Value of α, becauſe f, g, p, q, will be given, from the Hypotheſis that will be choſen, for the Variation of the Denſity in the internal Parts of the Spheroid.

XX. And if on the contrary α be given, that is, if we know by Obſervation the Ratio of the Axes of the Planet concerned; then by the foregoing Equation we may perceive, whether we have aſſumed an agreeable Hypotheſis for the Variation of the Denſities: But we cannot preciſely determine what this Hypotheſis muſt be, becauſe there is but one Equation, in which 4 indeterminate Quantities f, g, p, q, are involved. And indeed there might be many more than 4 indeterminate Quantities, if we ſhould aſſume more than two Terms in the general Equation of the Denſities $D=fr^p+gr^q+hr^s$, *&c.*

XXI. In order to apply the foregoing Theory to the Earth, it might ſeem at firſt Sight, that by the Aſſiſtance of Obſervations made for meaſuring the Length of the Pendulum, we might have other Equations, which with the foregoing Equation A, would determine the Coëfficients and Exponents now mentioned; but we ſhall ſoon ſee the Impoſſibility

* Elem. Aſtron. Lib. 3. *Sect.* 8. *Prop.* 52. † See Chap. III. of this Vol.

of this upon two Accounts: First, There need be only two Observations, as to what concerns the Length of the Pendulum. For because by Art. XVII. the Augmentation of the Gravity from the Equator to the Pole is proportional to the Square of the Sine of the Latitude, two Observations as much determine the Problem as an infinite Number can do: So that we could have but one other Equation besides the foregoing. This Equation will be

$$(B)\ \frac{\acute{p}-p}{p}=\frac{\dfrac{\overline{5-p}\,f\alpha}{\overline{3+p}\times\overline{5+p}}+\dfrac{\overline{5-q}\,g\alpha}{\overline{3+q}\times\overline{5+q}}}{\dfrac{\overline{p-1}\,f\alpha}{\overline{3+p}\times\overline{5+p}}+\dfrac{f}{3+p}+\dfrac{g}{3+q}+\dfrac{\overline{q-1}\,g\alpha}{\overline{3+q}\times\overline{5+q}}}.$$

The first Member of this Equation expresses the Gravity at the Equator subtracted from that at the Pole, and divided by that at the Equator; a Quantity which may be known in Numbers, by determining the Length of the Pendulum at two different Latitudes. The other Member of the Equation is an Expression of the same Quantity, as it is deduced by the preceding Calculus.

Secondly, This new Equation B cannot be of any Service in determining the Coëfficients and Exponents f, g, p, q, *&c.* For we shall now shew, that the foregoing Ratio $\frac{\acute{p}-p}{p}$ has such an immediate Connexion with α, that one of them being determined, the other will necessarily be so too, independently of the Values of f, g, p, q, *&c.* This may deserve our Attention, and the Proof is thus:

XXII. Because the Ratio of the Gravity to the Centrifugal Force is very great, and is expressed by m, in the Equation A we may reject the third and fourth Terms; by which means the Equation will be reduced to this, $\frac{f}{3+p}+\frac{g}{3+q}=\frac{4mf\alpha}{\overline{3+p}\times\overline{5+p}}+\frac{4mg\alpha}{\overline{3+q}\times\overline{5+q}}$.

And if from this Equation we deduce the Value either of f or g, and substitute it in the Equation B; (having first rejected the first and fourth Terms of the Denominator, as in this Case may be done) we shall have after the Calculation is made, whatever is the Number of Terms in the Equation of the Densities, $\frac{\acute{p}-p}{p}=\frac{10}{4m}-\alpha$, or $\frac{\acute{p}-p}{p}=\frac{1}{115}-\alpha$, by putting 288 for m, as has been long known. It is

The Figure of the Spheroid being known, the Augmentation of Gravity from the Equator to the

easily

Pole will be known alſo; and ſo vice verſa.

eaſily ſeen from this Equation, that when α is determined, $\frac{p' - p}{p}$ will be ſo too, which was the thing propoſed to be proved.

XXIII. But from this Equation there follows a very ſingular Propoſition, and which, in ſome ſort, is contrary to the Sentiments of Sir *I. Newton**, that *if by Obſervation it ſhall be diſcovered, that the Earth is flatter than according to the Spheroid of Sir* Iſaac, *that is, if the Diameter of the Equator exceeds the Axis by more than the* $\frac{1}{230}$ *Part, the Gravity will increaſe leſs from the Equator towards the Pole, than according to the Table which he has given for his Spheroid*; Prop. XX. of the 3d Book. And on the contrary, *if the Spheroid is not ſo flat, the Gravity will increaſe more from the Equator towards the Pole.*

XXIV. 'Tis thus that Sir *I. Newton* expreſſes himſelf about it, when he relates the Experiments made towards the South, concerning the Diminution of Gravity, which Experiments make it greater than his Theory requires†. He affirms, that the Earth is denſer towards the Centre than at the Superficies, and more depreſſed than his Spheroid requires. But by the foregoing Theory we may eaſily perceive, that if the Denſity of the Earth diminiſhes from the Centre towards the Superficies, the Dimunition of Gravity from the Pole towards the Equator will be greater than according to Sir *Iſaac*'s Table; but at the ſame time the Earth will be not ſo much depreſſed as his Spheroid requires, inſtead of being more ſo, as he affirms. Yet I would not by any means be underſtood to decide againſt Sir *Iſaac*'s Determination, becauſe I cannot be aſſured of his Meaning, when he tells us, that the Denſity of the Earth diminiſhes from the Centre towards the Circumference. He does not explain this, and perhaps inſtead of the Earth's being compoſed of parallel Beds or *Strata*, it's Parts may be conceived to be otherwiſe arranged and diſpoſed, ſo as that the Propoſition of Sir *Iſaac* ſhall be agreeable to the Truth.

XXV. As to Dr *Gregory*, who has attempted to comment upon this Paſſage of Sir *Iſaac*, I think I have demonſtrated, that he has committed a Paralogiſm. He ſays ‖ that if the Earth is denſer towards the Centre, or if (for Example) it has a Nucleus of greater Weight than the other Parts, the Diminution of Gravity from the Pole towards the Equator ſhall be greater than if the whole were of the ſame Denſity; and in this he is right. But he is in the wrong (I think) immediately to conclude from thence, that the Earth has a greater Flatneſs. Whence can he conclude this? It can be only from that Propoſition of Sir *Iſaac*

* *Princip. Math.* Ed. 3. p. 430. † *Et exceſſus longitudinis Penduli Pariſienſis ſupra longitudines Pendulorum iſochronorum in his latitudinibus obſervatas, ſunt paulo majores quam pro Tabula longitudinum Penduli ſuperius computata. Et propterea Terra aliquanto altior eſt ſub æquatore, quam pro ſuperiore, calculo, & denſior ad centrum quam in fodinis prope ſuperficiem.* ‖ *Elem. Aſtron.* Lib. 3. §. 8. Prop. 52. Schol.

which informs us, that Gravity is in a reciprocal Ratio of the Diſtances; becauſe he gave us the Propoſition but the Page before, as a Method for determining the Figure of the Earth. But we are not allowed to make uſe of this Propoſition in this Caſe, becauſe it has been ſhewn, Art. XVIII. that it can take Place only on the Suppoſition of a homogeneous Spheroid. Therefore, *&c.*

XXVI. It will not be very difficult, without any Regard had to the foregoing Theory, to find the Ratio of the Axes of a Spheroid, which we may ſuppoſe to have a Nucleus at the Centre, of greater Denſity than the reſt of the Planet; and hence we ſhall be eaſily aſſured of Dr *Gregory*'s Miſtake.

XXVII. Setting aſide all Attraction of the Parts of Matter, if the Action of Gravity is directed towards a Centre, and is in the reciprocal Ratio of the Squares of the Diſtances, the Ratio of the Axes of the Spheroid will then be that of 576 to 577: And the Gravity at the Pole, is greater than at the Equator by $\frac{1}{144}$th Part, or thereabouts. Which may be a Confirmation of what is here advanced, eſpecially to ſuch as will not be at the Pains of going through the foregoing Calculations. For we may conſider the Spheroid now mentioned, in which Gravity acts in a reciprocal Ratio of the Squares of the Diſtances, as compoſed of Matter of ſuch Rarity, in reſpect of that at the Centre, that the Gravity is produced only by the Attraction of the Centre or Nucleus.

XXVIII. In the foregoing Calculations, in order to find the Axes of our Spheroids, and to know whether their Figure makes a ſenſible Approach to that of the conical Ellipſis, we have had Recourſe to this Principle, that Gravity ought always to act in a Direction perpendicular to the Surface. Two Reaſons have prevailed with us to make uſe of this Principle rather than the other, which conſiſts in the Equilibrium of the Columns. The firſt is, becauſe the Calculations founded thereon are more ſimple. The ſecond is, that conſidering the ſtate of the actual Solidity of the Earth, it ſhould ſeem as if this Principle were the more indiſpenſably neceſſary. However, becauſe Sir *I. Newton*, and all the other Philoſophers, who have treated about the Figure of the Earth, have taken it, as it were, at it's firſt Formation, at which Time they ſuppoſe it to have been fluid; we ſhall here make the ſame Suppoſition, and we ſhall aſſume no other Ratio for that of the two Axes, than that of the Spheroid, which reſults from a Coincidence of theſe two Principles.

Fig. 41. We ſhall begin by inquiring what is the entire Weight of any Column CN. To do this we muſt reſume the Expreſſion of the Attraction in any Point M of the Column CN; then multiply it by $r + \lambda r$, and by the Denſity $f r^p + g r^q$, and afterwards we muſt find

the Fluent. Thus we shall have $\frac{cf^2 e^{2+2p}}{\overline{1+p}\times\overline{3+p}} + \frac{cg^2 e^{2+2q}}{\overline{1+q}\times\overline{3+q}}$

$$+ \frac{2cfge^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}} + \frac{2cfge^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}} + \frac{4cf^2\alpha e^{2+2p}}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}}$$

$$+ \frac{4cg^2\alpha e^{2+2q}}{\overline{1+q}\times\overline{3+q}\times\overline{5+q}} + \frac{8cfg\alpha e^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+q}}$$

$$+ \frac{8cfg\alpha e^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}} + \frac{\overline{4+2p}\,cf^2\lambda e^{2+2p}}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}}$$

$$+ \frac{\overline{4+2q}\,cg^2\lambda e^{2+q}}{\overline{3+q}\times\overline{5+q}\times\overline{1+p}} + \frac{\overline{8+4p}\,cgf\lambda e^{2+p+q}}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}}$$

$+ \frac{\overline{8+4q}\,cfg\lambda e^{2+p+q}}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}}$ for the total Gravity of any Column CN, having Regard only to the Attraction.

XXIX. If in this Expression we make $\lambda = o$, we shall have the Gravity of the Column at the Pole.

XXX. And if we make $\lambda = \alpha$, we shall have the Aggregate of the Attractions of the Column at the Equator.

XXXI. Now because the Column CN is *in Æquilibrio* with the Column CB; it follows from thence, that if we subtract the Weight of the Column CB, from the Aggregate of the Attractions of the Column CN, the Residue must be equal to the Sum of the centrifugal Forces of the Column CN. Now to endue our Spheroids with this Property, we will resume the Expression of the centrifugal Force in E, which we found Art. XIV. which will give $\left(\frac{8cfe^{1+p}\lambda}{\overline{3+p}\times\overline{5+p}} + \frac{8cge^{1+p}\lambda}{\overline{3+q}\times\overline{5+q}}\right)\frac{r}{e}$, for that Part of the centrifugal Force which acts according to CM, in any Place M, by expunging the Terms in which $\alpha\alpha$ would be found. This Value being multiplyed by $\dot{r}$, and by the Density, will give (when we have taken the Fluent) $\frac{8cf^2e^{2+2p}\lambda}{\overline{2+p}\times\overline{3+p}\times\overline{5+p}}$

$$+\frac{8cfge^{2+p+q}\lambda}{\overline{2+p}\times\overline{3+q}\times\overline{5+q}}+\frac{8cfge^{2+p+q}\lambda}{\overline{2+q}\times\overline{3+p}\times\overline{5+p}}$$

$+\dfrac{8cg^2e^{2+2q}\lambda}{\overline{2+q}\times\overline{3+q}\times\overline{5+q}}$ for the Sum of the centrifugal Forces of the Column CN, still expunging those Terms in which either $\alpha\alpha$ or $\lambda\lambda$ are found.

Then making this Expression equal to $\dfrac{\overline{4+2p}\,cf^2e^{2+2p}\lambda}{\overline{1+p}\times\overline{3+p}\times\overline{5+p}}$

$+\dfrac{\overline{8+4p}\,cfge^{2+p+q}\lambda}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}}+\dfrac{\overline{8+4q}\,cfge^{2+p+q}\lambda}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}}$

$+\dfrac{\overline{4+2q}\,cg^2e^{2+2q}\lambda}{\overline{1+q}\times\overline{3+q}\times\overline{5+q}}$, which is the Difference of the Weight of the Column at the Pole CB, from the Sum of the Attractions of the Column CN, we shall have the Equation $\dfrac{ppff}{\overline{1+p}\times\overline{2+p}\times\overline{3+p}\times\overline{5+p}}$

$+\dfrac{2pqfg}{\overline{2+p+q}\times\overline{3+p}\times\overline{5+p}\times\overline{2+q}}+\dfrac{2pqfg}{\overline{2+p+q}\times\overline{3+q}\times\overline{5+q}\times\overline{2+p}}$

$+\dfrac{qqgg}{\overline{1+q}\times\overline{2+q}\times\overline{3+q}\times\overline{5+q}}=o$, where we have put $e=1$, for the greater Simplicity of Calculation.

Determination of such Spheroids, as make the Principle of the Equilibrium of the Columns, and that of Gravity perpendicular to the Surface, to coincide with each other.

XXXII. This Equation informs us, that when out of all the infinite Varieties, which will be supplyed by the Equation of the Densities $D=fr^p+gr^q+hr^s$, *&c.* we shall have taken at Pleasure all the Coëfficients, and all the Exponents, one only excepted; if this last is such in respect of the others, that it may fulfil the Conditions of the foregoing Equation, the Spheroid, being supposed in a State of Fluidity, will be *in Æquilibrio*, because it will unite as well the Principle of a perpendicular Tendency to the Surface, as that of an Equipoise of the several Columns.

XXXIII. Before I conclude this Paper, I shall make a few Reflections on the Principles we have now made use of, for determining the Figure of a Spheroid revolving about it's Axe.

The first Principle which, after Mr *Huygens*, we have had Recourse to, and which consists in making Bodies gravitate perpendicularly to

to the Surface, seems to me of absolute Necessity. For if there were never so little Water upon the Surface of the Earth, it could not be at Rest, if it had a Tendency any how inclined to the Surface.

The second Principle made use of by Sir *I. Newton*, and which consists in an Equilibrium of the Columns C E, C N, C P, could be thought necessary (I think) only for these two Reasons: The first is that which is usually assigned, that at the first Formation of the Earth, it was probably in a State of perfect Fluidity; in which case it must acquire such a Figure, as will result from the Equilibrium of the Columns, and from the Gravitation acting perpendicularly to the Surface. Indeed though this Reason has a Degree of Plausibility, yet there are many who think it to be of small Force. Perhaps, say they, the Earth has never been in this fluid Condition.

The second Reason, which I believe will have a greater Weight with every Body is this. Considering the Earth as it is at present, and without carrying our Thoughts so far back as to it's Formation, if the Ocean, which is now upon it's Surface, has any considerable Depth, and if it's Parts preserve a Communication with each other, from Region to Region, by subterraneous Canals; it can only keep an Equilibrium by this Means, because it's Superficies is the same as it would have, were the whole a Fluid.

XXXIV. This second Reason has suggested a Reflexion to my Mind, concerning the Equipoise of the Columns now calculated, Art. XXXI. and XXXII. Let us first suppose, that the Earth is our fluid Spheroid, composed of Beds of different Densities; and that afterwards this Fluid hardens into a Solid, so that the different Beds or *Strata*, of which it is made up, are of no other Use but to cause a Gravity by their Attractions. Then let us suppose, that the Seas and great Waters about the Earth have a Communication with each other, by means of some subterraneous Canals. As the Waters of the Sea, which unite with one another, are probably homogeneous, the foregoing Calculation, wherein we have considered the Spheroid as a Fluid, can no longer take Place, because we have there supposed, that the Fluid contained in the Canal B C N is of a Density, that varies from the Center to the Circumference. From hence it seems to me, we must undertake the Computation of the Equilibrium of the Columns after another Manner, thus:

We must examine whether two Canals, as C N and B C, which are filled with a homogeneous Fluid, will be *in Æquilibrio*, all the other Parts of the Spheroid continuing as above.

XXXV. To do this, we will begin with finding the Gravity of any Column C N, arising from Attraction alone. First, then, we must resume the Expression of the Attraction in any Point M, Art. VII. Then we must multiply it by $\dot{r} + \lambda \dot{r}$, which will give Fig. 41.

$$\frac{2cfr^{1+p}\dot{r}}{3+p} + \frac{\overline{8+4p}\,cf\lambda r^{1+p}\dot{r}}{\overline{3+p}\times\overline{5+p}} + \frac{8cf\alpha r^{1+p}\dot{r}}{\overline{3+p}\times\overline{5+p}}$$

$+ \dfrac{2cgr^{1+q}\dot{r}}{3+q}$ *&c.* And taking the Fluent of this Quantity, we shall have $\dfrac{2cfe^{2+p}}{\overline{3+p}\times\overline{2+p}} + \dfrac{4cf\lambda e^{2+p}}{\overline{3+p}\times\overline{5+p}}$

$+ \dfrac{8cf\alpha e^{2+p}}{\overline{2+p}\times\overline{3+p}\times\overline{5+p}} + \dfrac{2cge^{2+q}}{\overline{3+q}\times\overline{2+q}}$, *&c.* for the Gravity of the whole Column C N.

XXXVI. If in this Value we make $\lambda = o$, we shall have the Gravity of the Column at the Pole.

XXXVII. And if we subtract the Gravity of the Column at the Pole from the whole Sum of the Attractions of the Column C N, we shall have $\dfrac{4cf\lambda e^{2+p}}{\overline{3+p}\times\overline{5+p}} + \dfrac{4cge^{2+q}\lambda}{\overline{3+q}\times\overline{5+q}}$, which must be equal to the Sum of the centrifugal Forces of the Column C N, in order that the Columns C B and C N may be *in Æquilibrio.*

But we shall find this really to obtain, if we resume the Quantity $\left(\dfrac{8cfe^{1+p}}{\overline{3+p}\times\overline{5+p}} + \dfrac{8cge^{1+q}\lambda}{\overline{3+q}\times\overline{5+q}}\right)\dfrac{r}{e}$, which expresses (Art. XXXI.) that Part of the centrifugal Force in M, which acts according to C M. Then multiplying this Expression by $\dot{r}$, and seeking the Fluent, we shall have $\dfrac{4cfe^{2+p}\lambda}{\overline{3+p}\times\overline{5+p}} + \dfrac{4cge^{2+q}\lambda}{\overline{3+q}\times\overline{5+q}}$ for the Aggregate of the centrifugal Forces of the Column C N. And this being the same as the foregoing, shews, that the Columns C B and C N are *in Æquilibrio*, supposing them to be homogeneous; nor are we here obliged, as in Art. XXXII. where we consider them as heterogeneous, to suppose the Coëfficients $f\,p$, &c. to have any certain Relation among one another.

XXXVIII. Perhaps it may be urged, that the foregoing Calculus agrees only to a Canal, as B C N, which passes through the Center; and that we ought to prove, in the same Manner, that the Water included

cluded in any other Canal $p\ q\ r$ would obſerve an *Æquilibrium.* But it appears to me, that this Property may be derived from the former: For it follows from the foregoing Calculation, that if we might be allowed to make this Hypotheſis, *viz.* That independently of the Attraction of any Matter, the Gravity at any Diſtance C N from the Center (See *Fig.* 41.) would be proportional to $\frac{2cfe^{1+p}}{3+p}$

$$+\frac{\overline{2p-2}\ cf\lambda e^{1+p}}{\overline{3+p}\times\overline{5+p}}+\frac{8cf\alpha e^{1+p}}{\overline{3+p}\times\overline{5+p}},$$ *&c.* it is plain from thence, that a Maſs of the homogeneous Fluid, which ſhould turn about the Axis C B, would aſſume the ſame Form as that of our heterogeneous Fluids. But if this Spheroid ſhould then put on a fixed State, except only ſome Canal $p\ q\ r$, the Water in this Canal would be *in Æquilibrio*; for without this, the Spheroid could not be eſteemed as having arrived to it's fixed State. But this Suppoſiton comes to the ſame as that of our heterogeneous Spheroid, compoſed of elliptical Beds, in which ſhould be found a Canal $p\ q\ r$ of a homogeneous Fluid; provided that the Space, which this Canal poſſeſſes in the Globe, be not of ſo large an Extent, as to change the Law of Attraction.

The only three Planets, in which we can be aſſured of Gravitation, and the centrifugal Force, are the Sun, Jupiter and the Earth. As to the Sun, the centrifugal Force is there ſo ſmall, in reſpect of it's Gravity, that his Poles muſt be very little depreſſed, ſo that we cannot be ſenſible of it by Obſervation. Then as to Jupiter, Obſervations make him ſomething leſs flat than according to Sir *I. Newton*; that is to ſay, than if he were compoſed of Matter of an uniform Denſity. Therefore by the foregoing Theory, he muſt be a little more denſe towards the Center, than at the Parts near the Superficies. We might make a thouſand Hypotheſes about the Manner of diſtributing the Inequality of Denſity, proceeding from the Center towards the Circumference, which would all agree with the Figure obſerved, and which are very eaſy to calculate by the Principles here laid down.

As to what concerns the Earth, I ſhall wait till we receive the Obſervations which muſt have been lately made in *Peru*; that by comparing thoſe with what Obſervations we have made under the arctick Circle, and with thoſe of Mr *Picart* in *France*, we may have the true Difference of the Earth's Diameters at the Equator and at the Poles. Then our Theory may be applied, to determine whether the Earth is more or leſs denſe at the central Parts than at the Surface, or whether it be every-where of an uniform Denſity, as it ought to be, if (without admitting very groſs Errors in the Obſervations) it may be concluded, that the Earth is really the Spheroid of Sir *I. Newton*; and this Caſe would be the ſimpleſt and the moſt natural of all.

 I am

I am here obliged to acknowledge, that if the Obſervations we have made in the North may be relied upon, and if we muſt admit as incontestible as well the Meaſure of a Degree as the Length of the Pendulum, the foregoing Theory could not be reconciled to the *Phænomena.* For it follows from our Obſervations, that the Diameter of the Equator muſt exceed the Earth's Axis by more than $\frac{1}{230}$ Part: And that the Gravity at the Pole muſt be greater than that at the Equator by more than $\frac{1}{230}$ Part likewiſe; which will by no means agree with what we have deduced in Art. XXIII.

As to what concerns the Meaſure of Gravity in *Lapland*, as being not ſo liable to Error as the meaſuring a Degree; the Earth may be not quite ſo flat as Sir *Iſaac*'s Spheroid requires. By the Table of the Length of the Pendulum, exhibited in the Treatiſe concerning the Figure of the Earth, publiſhed this Year by M. *de Maupertuis*, and by Art. XXII. of the preſent Diſcourſe, the Earth may be more elevated at the Equator than at the Pole by the $\frac{1}{266}$ Part, or thereabouts. After the true Quantity of the Earth's Flatneſs ſhall be fully ſettled, if it ſhould be found to have this Figure, I ſhould be apt to think it is a little more denſe at the Center than towards the Superficies. But if, on the contrary, we ſhould be well aſcertained, that the Earth is raiſed higher at the Equator than at the Pole, by above the $\frac{1}{230}$ Part; and if, for any ſufficient Reaſon, we may ſomething ſhorten the Length of the Pendulum that beats Seconds in the North; there would be ſome Grounds to allow, that the Earth is not ſo denſe at the central Regions as at thoſe near the Surface. But if it ſhall happen, that we can neither diminiſh the Length of the Pendulum, nor the Exceſs of the Equatorial Diameter above the Axe, I muſt then give up my Hypotheſis.

Of a Curve called from it's Figure, a Cardioïde, *by* Joannes Caſtillioneus. No. 461. p. 778. *Aug.* &c. 1741. Fig. 42, 43, 44.

X. The Diameter B A of the Semicircle B M A, touching the Circumference in the Point B, ſo as always to paſs over the Point A, will generate the Curve in queſtion. From the *Geneſis* it appears,

1. That D A α perpendicular to A B is equal to double the Diameter.

2. That the Periphery of this Curve A D N $a\ \alpha$ N A will terminate in A.

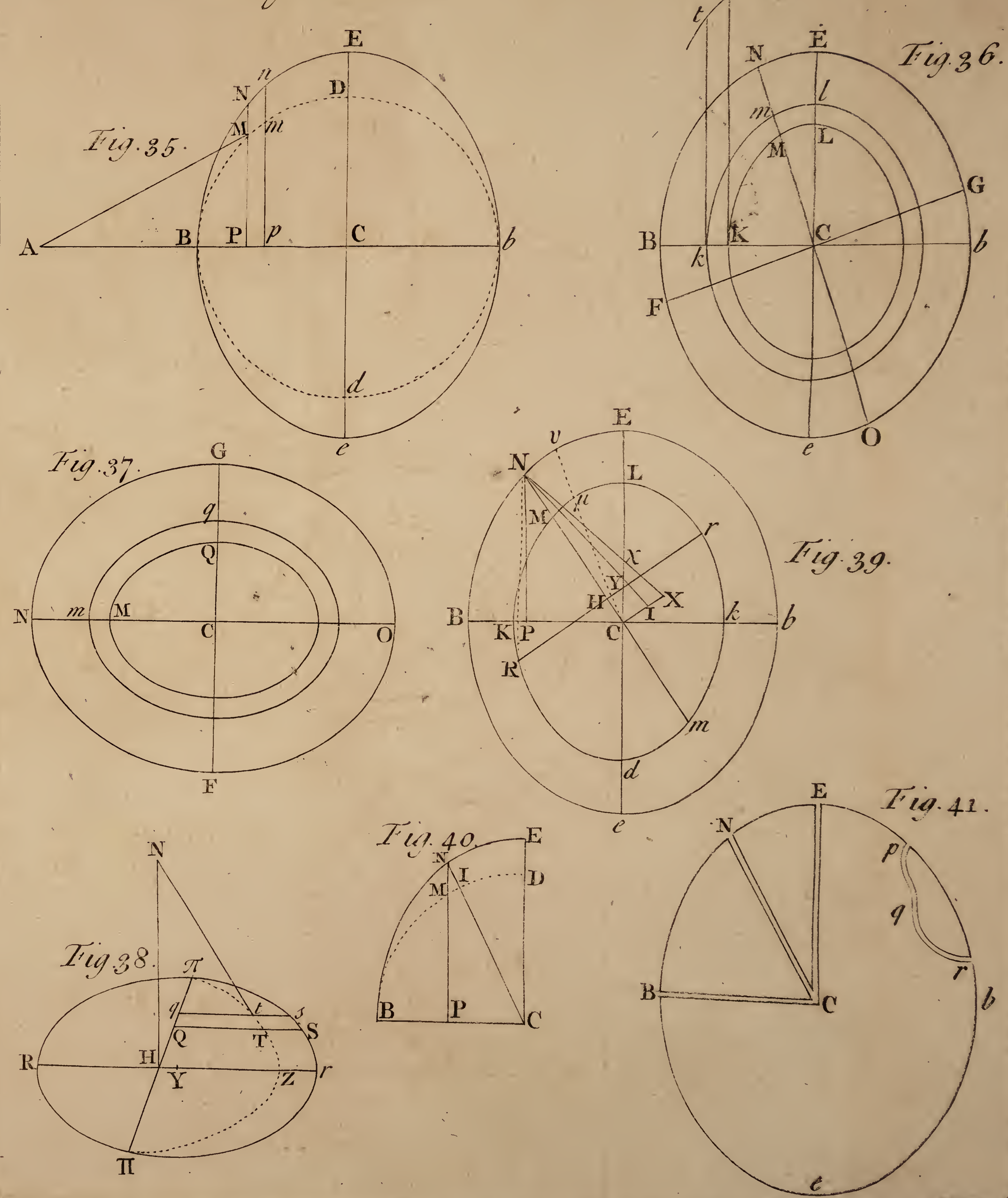
We may call this Curve, from it's Figure, a *Cardioïde.*

Now through a and A draw a E, A Q, perpendicular to a A; and E N perpendicular to a E. It follows from the *Geneſis*, that A N = B A + A, and (by the Similitude of the Triangles Q A N, M B A) A Q = B M + M P, and N Q = M A ± A P.

This is the chief Property of our Curve, and there is another, which is no unpleaſant one, that the right Line N N is always equal to double the Diameter, and is always biſected by the Circle in M.

Now let B A $= a$, a E $= x$, E N $= y$, then Q N $= \mp y \pm 2a$; A N $= \sqrt{x^2 + y^2 - 4ay + 4a^2}$, and M A $= \mp a \pm \sqrt{x^2 + y^2}$

Pl. a. V. Vol. VIII. part 1st page 108.



J. Mynde sc.

$\sqrt{x^2 + y^2 - 4ay + 4a^2}$, which 4 Lines being compared by Analogy, give the Equation to the Curve,

$$y^4 - 6ay^3 \left.\begin{matrix} + \; 2x^2y^2 - 6ax^2y + x^4 \\ + \; 12a^2y^2 - 8a^3y + 3a^2x^2 \end{matrix}\right\} = 0.$$

The Subtangent of the Curve, according to the common Methods, is, $\dfrac{2y^4 - 9ay^3 + 2x^2y^2 + 12a^2y^2 - 3ax^2y - 4ay^3}{6axy - 2xy^2 - 3a^2x - 2x^3} = \dfrac{x}{y}$.

But a more eaſy Method of drawing the Tangent may be deduced from the Generation of the Curve. Let M A N come into the neareſt Place to the firſt *m* A *n*, take A R = A M, and A *r* = A N, and having joined M R, N *r*, draw through A the Right Line A T parallel to them, and through M *m*, N *n*, the Right Lines M T, *n t*. Now *n* A : A *t* : : *n r* (or *m* R) : *r* N : : *m* R × M A : *r* N × A M : : *m* R × M A : M R × A N : : M A × A *m* : A N × A T, but in the laſt *Ratio m* A = M A, and T A perpendicular to M N, wherefore *n* A : A *t* : : $\overline{MA}^2$: A N × A T ; now if from M be drawn through the Center of the Circle F, the Right Line M F, to be produced till it meets the Right Line produced alſo in G ; that is, to the Periphery of the Circle, then MA^2 = T A × A G ; wherefore *n* A : A T : : A G : A N ; therefore let a Semicircle be deſcribed through G and N, which will cut the Right Line A T in *t*, from which the Right Line *t* N being drawn, will be a Tangent to the Curve, to which alſo the Right Line N G is perpendicular ; from hence let M O be joined, to which draw a parallel from N touching the Curve.

Here let us obſerve by the way, that this Method of drawing Tangents agrees with moſt Curves.

Let A B be a Conchoide of *Nicomedes* : then, ſuppoſing the former Preparation, B P : P *t* : : $\overline{BR}$ (or *c r*) : R *b* : : *c r* × C P : R *b* × C P (or *r* C × P R) : : $\overline{CP}^2$: T P × P R, whence the former Conſtruction is deduced. Fig. 43.

Let a Right Line of a given Length C P B, touching the Right Line C D T perpendicular to D A, at the Point C, always paſs over a given Point P in D A, and ſo generate the Curve A B. Fig. 44.

If you apply the former Preparation and Reaſoning to this, you will have B P : P *t* : : *b* R (*r c*) : R B : : *c r* × C P : R B × C P (B P × *r* C) : : CP^2 : B P × P T, as before.

But the Method *de maximis & minimis* gives the greateſt Ordinate $= \dfrac{a}{4}$, and it's Abſciſs $= \dfrac{a}{4}\sqrt{3}$. In the ſame Manner the greateſt Abſciſs might be inveſtigated ; but this would be tedious ; therefore ſeek it thus.

Becauſe E N is a Tangent to the Curve, the Right Line M G drawn from the Point M thro' the Center F determines the Point G, from Fig. 42.

from which G N being drawn is perpendicular to E N, therefore alfo to A a, by the Hypothefis, but N Q = A V = M A + A P; therefore V P = M A; but B A : A M : : M A : A P; therefore B A : P V : : V P : P A; but P F = F V = $a - 2z$; and therefore $a : a - 2z :: a - 2z : z$. Hence is eafily deduced $z = \frac{a}{4}$, E N = $\frac{7a}{4}$, A Q = $\frac{3a}{4}\sqrt{3}$. Here we muft obferve, that the fame Point M, which affords in the Right Line N A M N the Point of the greater Ordinate, affords alfo the Point of the greater Abfcifs.

A Rule for finding the meridional Parts to any Spheroid, with the fame Exactnefs as in a Sphere, by Colin Mac Laurin, *F. R. S. Communicated by* Andrew Mitchel, *Efq; F R. S.* No. 461. p. 808. *Aug.* &c. 1741.

XI. It was demonftrated long ago, that in a Sphere the Nautical Meridian Line is a Scale of logarithmic Tangents of the half Complements of the Latitudes. The fame may be computed with no lefs Exactnefs to any Spheroid by the following Rule.

Let the Semidiameter of the Equator be to the Diftance of the *Focus* of the generating Ellipfe from the Center as m to 1. Let A reprefent the Latitude for which the meridional Parts are required, s the Sine of this Latitude, the *Radius* being Unit; find the Ark B, whofe Sine is $\frac{s}{m}$; take the logarithmic Tangent of half the Complement of B from the common Tables; fubtract this logarithmic Tangent from 10.000000, or the logarithmic Tangent of 45°; multiply the Remainder by $\frac{7915.7044678978}{m}$, *&c.* and the Product fubtracted from the meridional Parts in the Sphere, computed in the ufual manner for the Latitude A, will give the meridional Parts expreffed in Minutes for the fame Latitude in the Spheroid, provided it is oblate. When the Spheroid is oblong, the Difference of the meridional Parts in the Sphere and Spheroid for the fame Latitude, is then determined by a circular Ark; but it is not neceffary to defcribe this Cafe at prefent.

Example: If $m\,m$: 1 : : 1000 : 22. then the greateft Difference of the meridional Parts in the Sphere and Spheroid is 76.0929 Minutes: In other Cafes it is found by multiplying the Remainder abovementioned by 1174.078.

CHAP.

Pla. VI. Vol. VIII. part 1.st page 110.

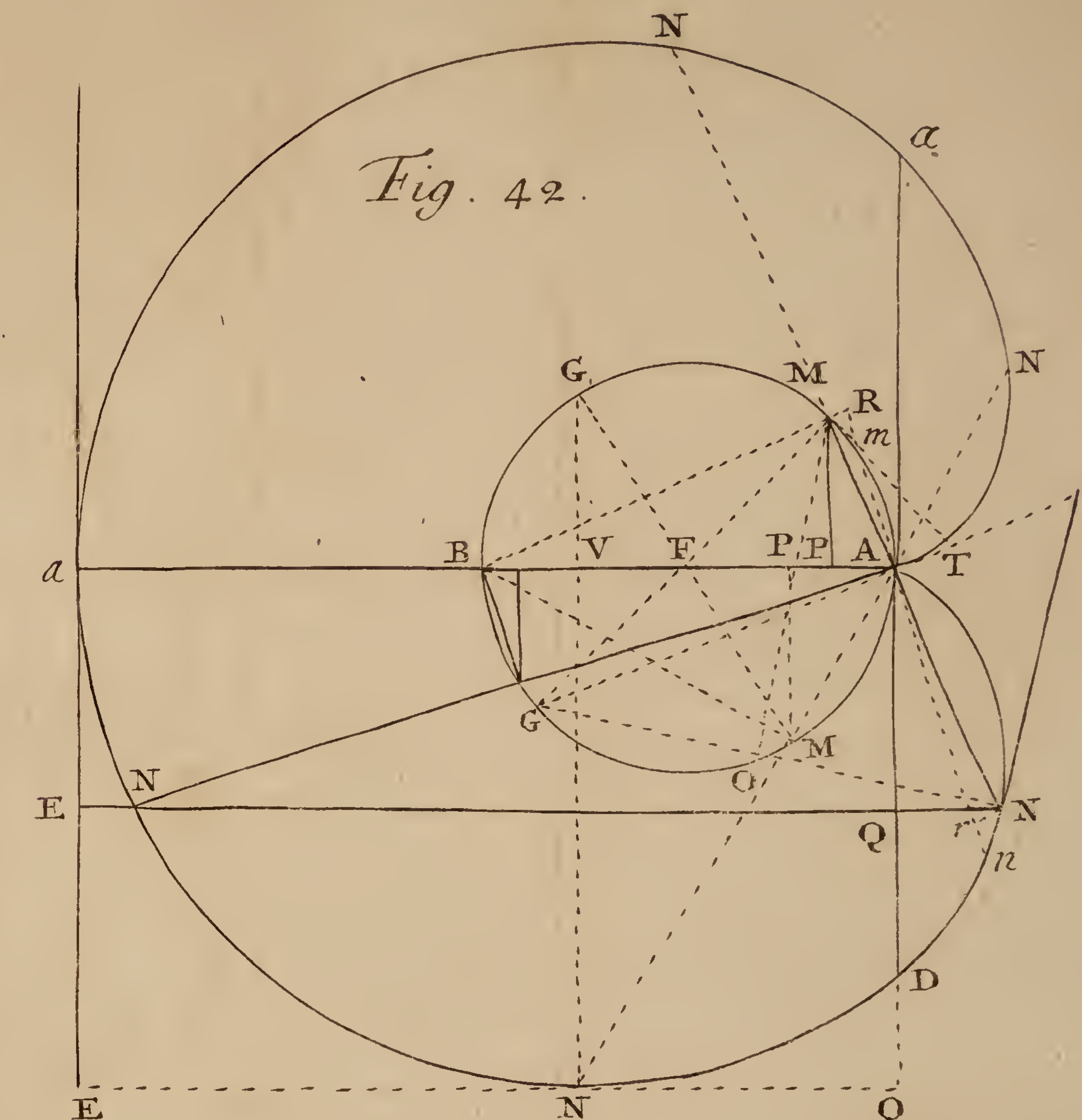


Fig. 42.

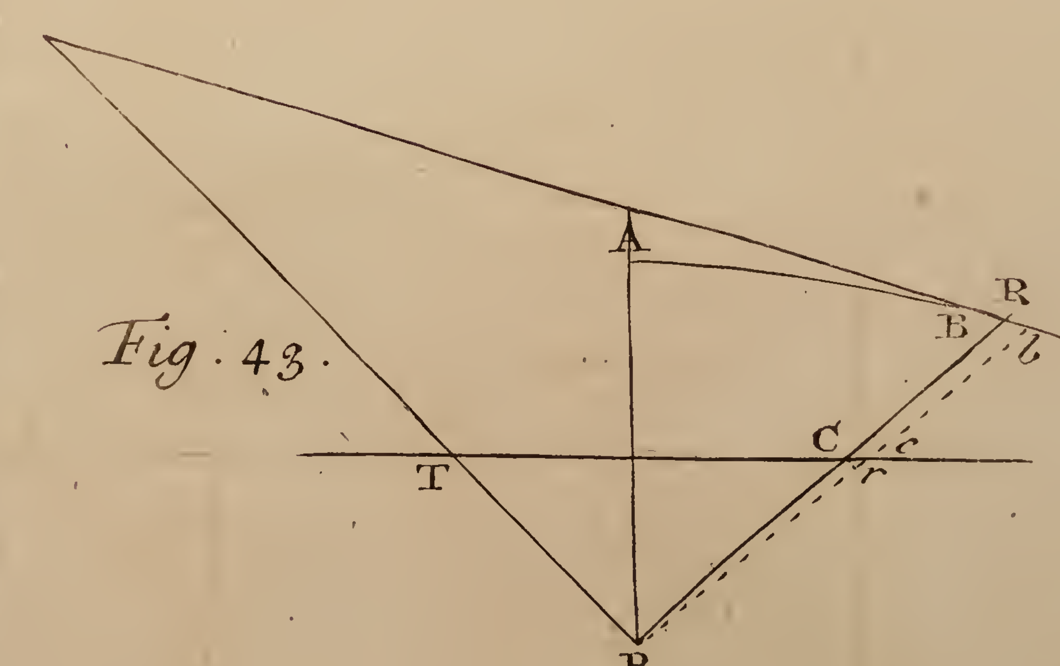


Fig. 43.

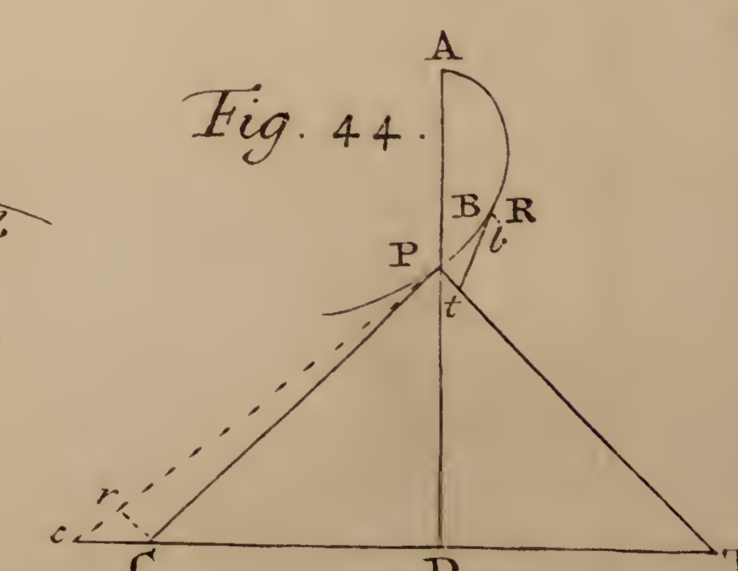


Fig. 44.

CHAP. II.

OPTICKS.

I. IF two Lens's of equal focal Length be put together in the Form of a Teleſcope, and a Plane Speculum be placed before one of them, ſo that the Axis of the Teleſcope make any Angle with it's Surface, and a Ray of Light (the Line of whoſe Direction lies in a Plane perpendicular to that Surface, and paſſing through the Axis of the Teleſcope) fall on it, and be reflected from it, ſo as to paſs thro' the Teleſcope; then the Line of it's laſt Direction, after paſſing the Teleſcope, will make an Angle with that of it's firſt Direction, before it's Incidence on the Speculum, very nearly equal to double the Angle made between the Axis of the Teleſcope, and the Surface of the Speculum.

A Propoſition relating to the Combination of Tranſparent Lens's with Reflecting Planes. By J. Hadley, *Eſq; V. Pr. R. S. Communicated* Jan. 9, 1734. No. 440. p. 185. *Jan.* &c. 1740.

LEMMA. Fig. 45.

Let the Line F G be the common Axis of the two Lens's I D and K E, of equal focal Lengths; to which let the Lines A D, D B and B E, be each equal; and let a Ray of Light, iſſuing from a Point in the Axis F, fall on the Lens I D at I, and be there refracted into the Line I G, cutting the Axis in G, and meeting the Lens K E in K, where let the Ray be again refracted into the Line K H, cutting the aforeſaid Axis in H: The Angles I F D and K H E are very nearly equal.

Demonſtration.

It is known from Dioptricks, that the Lines F I, I G, K H, and F G, are all in the ſame Plane; and by the Conſtruction the Lines A D, D B, and B E are equal; and by *Prop.* 20 of *Huygens*'s *Dioptricks*, the Lines F A, F D, and F G are continually proportional; and conſequently F A : A D : : F D : D G, and dividing, F A : A D : : F D — F A (= A D) : D G — A D (= B G.) Therefore A D : B G : : F D : D G. By the ſame *Propoſition*, the Lines B G, E G, and H G are alſo continually proportional, and B E (= A D) : B G : : E H : E G. Hence it follows, that the Lines F D, D G, E H, E G, are Proportionals. But as F D is to D G, ſo is the Tangent of the Angle I G D or K G E to the Tangent of the Angle I F D; and as E H is to E G, ſo is the Tangent of the Angle K G E to the Tangent of the Angle K H E. The Tangent of the Angle K G E therefore has the ſame Proportion to the Tangents of each of the Angles I F D and K H E, and conſequently thoſe Angles are equal. *Q. E. D.*

N. B. In the Demonſtration of the above-cited Prop. of *Huygens*, the Thickneſs of the Lens's are neglected, and the Diſtance of the Points I and K, from the Line F G, ſuppoſed very ſmall; ſo that if either-

either of thofe are too great, there may arife a fenfible Difference between the Angles I F D and K H E.

Fig. 46. Let D F and C G reprefent the two Lens's put together as before, having their common Axis in the Line E L, and B N a plane Speculum to which that Line is inclined in the Angle G H N, and let A B be a Ray of Light falling on the Speculum at B, as is before expreffed, and let it be there reflected towards the Point C of the Lens C G, where it is refracted towards the Point D of the Lens D F, and there again refracted into the Line D E, cutting the Axis in E. The Angle A O P contained between this laft Line D E, continued backwards, and the firft Line of Incidence of the Ray A B, will be very nearly equal to double the Angle of Inclination of the Axis of the Lens's E L to the Plane of the Speculum B N; *i. e.* double the Angle G H N.

Demonftration. Produce the Lines of Incidence and Reflection of the Ray A B and B C, till they meet the Axis of the two Lens's in I and L; and thro' the Point B draw B K perpendicular to the Plane of the Speculum, and cutting the fame Axis in K, the Angles K B L and K B I are equal. The Angle K L B is the Difference of the Angles I K B and K B L; and the Angle H I B is the Sum of the Angles I K B and K B I (= K B L): Therefore the Angle I K B is equal to half the Sum of the Angles H I B and K L B. But by the foregoing *Lemma*, the Angles K L B and F E D are very nearly equal. Therefore the Angle I K B is nearly equal to half the Sum of the Angles H I B, and F E D; that is to half the Angle P O B, and it's Complement B H I or G H N is nearly equal to half the Angle A O P the Complement of P O B to a Semicircle. *Q. E. D.*

If the firft Incidence of the Ray be fuppofed to be in the Line E D, it will proceed in the fame Track as before, but with the contrary Directions; fo that the Angle E O B made between the firft incident Ray and the laft reflected, will ftill be equal to the Double of G H N, as before.

It is evident that on this Principle an Inftrument might be conftructed, the Effects of which would in a great Meafure refemble thofe of that before mentioned *: But it would be liable to the Errors arifing both from the fpherical Figure of the Lens's, and alfo the different Refrangibility of the Rays of Light, when the Object is feen at a Diftance from the Axis of the Telefcope; altho' thofe Errors, by a proper Difpofition of the Parts of the Inftrument, may be reduced to a very fmall Quantity. However, for this Reafon, and alfo becaufe the Inftrument feemed to me to be attended with greater Inconveniencies, both in it's Conftruction and Ufe, than the other, I have not thought it neceffary to give any more particular Defcription of it.

* See Vol. VI. p. 139.

A new Method of improving and perfecting Catadioptrical Telescopes, by forming the Speculums of Glass instead of Metal. By Mr Caleb Smith. No. 456. p. 326. *Jan.* &c. 1740.

II. The Imperfections of Telescopes are attributed to two Causes; The Unfitness of the Spherical Figure to which the Glasses are usually ground, and the different Refrangibility of the Rays of Light.

The first of these Defects only, was known to the Writers of Dioptrics, before Sir *I. Newton*; for which Reason (as he informs us himself*, they 'imagined, that Optical Instruments might be brought 'to any Degree of Perfection, provided they were able to communicate 'to the Glasses, in grinding, what Geometrical Figure they pleased; 'to which Purpose various Mechanical Contrivances were thought of, 'whereby Glasses might be ground into Hyperbolical, or even Parabolical, Figures; yet nobody succeeded in the exact Description of 'such Figures; and had their Success been answerable to their 'Wishes, yet their Labour would have been lost, for the Perfection 'of Telescopes is limited, not so much for want of Glasses truly figured, 'according to the Prescriptions of Optic Authors, (which all Men 'have hitherto imagined) as because that Light itself is an heterogeneous 'Mixture of differently refrangible Rays; so that were a Glass so 'exactly figured as to collect any one sort of Rays into one Point, 'it could not collect those also into the same Point, which having 'the same Incidence upon the same Medium, are apt to suffer a 'different Refraction †.' And again,——'The different Refrangibility 'of different Rays, is an obstruction to the perfecting of Optical Instruments, either by Spherical or other Figures; and unless the Errors 'thence arising, can be corrected, all the Labour spent in correcting the 'rest will be to no purpose ‖.

Now, for this principal and last-mentioned Defect, no one, that we know of, has proposed any Remedy; apprehending, perhaps, the Difficulty of attaining such to be insuperable; inasmuch as the great Author of this Discovery, himself, had not shewed us any Method whereby to correct those Errors which arise from this Inequality of Refraction; but rather discouraged any such Attempts, by declaring, 'that on this Account he laid aside his Glass-works**, and looked upon 'the Improvement of Telescopes, of given Lengths, by Refraction, 'as desperate ††.'

However, as it has been proved by incontestable Experiments, that this Dissipation of the Rays of Light, from whatever Cause it proceeds, in passing out of one Medium into another, is not accidental and irregular; but that every sort of homogeneal Rays whether more or less refrangible, considered apart, are refracted according to some constant uniform and certain Law; and as the removal of so great an Impediment as this of unequal Refraction in the Rays of Light, is of great Importance to the Science of Dioptrics, and absolutely necessary to it's further Advancement; we have thought it worthy

* *Opt. Lect.* 1. 2. † *Phil. Trans.* No. 80. ‖ *Princip. Schol. ad fin.* Lib. 1.
** *Phil. Trans.* No. 80. †† *Opt. Ed.* 2. *p.* 91.

of a careful Examination, whether, in some Cases at least, it might not be possible for contrary Refractions so to correct each other's Inequalities, as to make their Difference regular; and if this could be conveniently effected, Sir *I. Newton* has acknowledged, 'there would 'be no farther Difficulty *.'

Now, upon a due Consideration of this subject, we have found it possible, by proper Methods and Expedients, to rectify those Errors which proceed from the different Degrees of Refrangibility in different Rays, passing from one Medium into another; admitting only this well-known and established Principle, upon which we ground our Reasoning, *viz.* 'That the Sines of Refraction of Rays differently 'refrangible, are one to another in a given Proportion, when their 'Sines of Incidence are equal †.' And our present Design is, to shew what Advantage this will yield towards improving and perfecting Catadioptrical Telescopes, by making the Speculums of Glass, instead of Metal, in the following Manner:

Fig. 47. Let A B C D E F represent the Section of a concavo-convex Speculum, whose two Surfaces are Segments of unequal Spheres; call the Radius of the Sphere, to which the concave Side is ground, a; and the Radius of the convex Surface, which must be quicksilvered over, e; let B R be the Axis of the Speculum, or a Line perpendicular to both the Surfaces; and therein let P be the principal Focus, or Point where parallel Rays of the most refrangible Kind are collected, by this Speculum; and Q the Focus, or point of Concourse, of such Rays as are least refrangible; to wit, after they have suffered two Refractions, at entering into, and passing out of, the concave Surface D E F, and also one Reflection from the convex Surface A B C: If the Radius of Concavity be greater than the Radius of Convexity, as we will in the first Place suppose, then P will fall nearer the Vertex of the Speculum than the Point Q; and the Interval Q P will be the greatest Aberration, or Error, occasioned by the Separation, or unequal Refraction, of the greatest and least refrangible Rays, after their Emergence from the concave Surface F E D. Call the common Sine of Incidence, n; the Sine of Refraction of the least refrangible Rays out of a dense Medium into a rarer, m; and, of the most refrangible, μ; then, according to the known and received Laws of Refraction and Reflection, the Focal Distance of the most refrangible Rays, from the Vertex of the Speculum, (neglecting it's Thickness, as of little or no Moment in the present Case) will be found $= \frac{n\ a\ e}{(a-e)\,2\,\mu + 2\,n\,e} =$ P B. And the Quantity of the greatest Aberration, occasioned by the different Refrangibility of the most and least refrangible Rays, P Q, will be to the focal Distance just mentioned, P B, as $(a-e(\times(\mu-m)$ to $(a-e)\,m+e\,n$; which

* *Phil. Trans.* No. 88. † *Opt. Ed.* 2. p. 66.

Quantity

Quantity, or Error, thus obtained, (to abbreviate the Calculation) call ε; and now let it be required to form a Lens, if possible, which placed at some given Point in the Axis between the Focus of the most refrangible Rays P, and the Vertex of the Speculum (as H), shall refract not only the Rays of the most refrangible Kind tending to the Point P, but also the Rays of the least refrangible Kind tending to Q, in such a Manner, that both Sorts shall concur, after such Refraction, in some other Point of the Axis R; let H P the given Distance of the Point in the Axis H, from the Focal Point P, be called d; and then if the Point H has been assumed, so that the said given Quantity, or Distance, d, is greater than $\frac{(\mu - n)\varepsilon}{\mu - m}$, but less than $\frac{m\varepsilon}{\mu - m}$, I say the refracting Superficies G H I, that shall perform what was required, will be part of a concave Sphere, whose Radius is $= \frac{(dd + d\varepsilon) \times (\mu - m)}{m\varepsilon - (\mu - m) d}$, and H R, the Distance of the given Point H, from R, the Point to which all the Rays will tend, after Refraction at the said concave Surface, (whose Radius being found, as above, we call υ) will be $= \frac{\mu d \upsilon}{(d + \upsilon) n - \mu d}$. Lastly, upon the Point R thus obtained, as a Centre, with an Interval a little less than H R, describe the circumference KLM, and the Figure GHIMLK will denote the Section of a double concave Lens, which, placed at the given Point in the Axis H, (taken nevertheless within the Limits above-mentioned) will collect all Sorts of Rays proceeding from the Speculum, into one and the same Focus, or Point of the Axis, R, as was required; for the Surface G H I, which first receives those Rays, will refract the most refrangible Sort converging to the Point P, and also the least refrangible converging towards Q, so that both Sorts, after such Refraction, will concur in the Point R; but the Rays tending to R, 'tis manifest, will suffer no Refraction at their Emergence from the Superficies K L M, because R is the Centre thereof, by Construction; which Point, R, where a perfect Image of an Object infinitely distant will be formed, we call the Focus of the Telescope, to distinguish it from the Point, P, which we have before called the Focus of the Speculum.

In this manner a Lens, (or instead thereof a triangular Prism with two of it's Sides ground concave, and the third plain, if that be found as practicable) may be formed and situated, so as to correct the Errors of the Speculum arising from the different Refrangibility of the Rays of Light. But, in order to render this kind of Telescopes absolutely perfect in their Construction, the Errors also that result from the spherical Figure must be rectified; and with regard to this, we assert, that it is possible to assume a Point in the Axis, between the

Focus of the Speculum and it's Vertex, (as we have taken the Point H, in the following Example*) at which, if a refracting Superficies, or Lens, be constituted, according to the Method already delivered, it will not only correct the Errors occasioned by the unequal Refraction of the Rays of Light, but also rectify such as proceed from the spherical Figure of this Speculum, to a much greater Degree of Exactness than is requisite for any Physical Purpose (meaning always the Errors of those Rays which respect the Axis). Now to find or determine this Point, affords a Problem not easy to be solved; and we recommend it, as worthy of the Consideration of Geometricians.

Seeing therefore it is possible, and we believe also practicable, to remedy the Imperfections of this kind of Speculums, (from whatsoever Cause they arise) by the Method we have here proposed; it seems to follow, that Catadioptrical Telescopes may be carried, by this means, to as great a Degree of Perfection, as they are capable of receiving; provided spherical Figures can be truly communicated, with an exquisite Polish, to Glasses of a large Aperture, and a Foil of Quicksilver made also to retain that Figure accurately, and without any Inequality; for the Object-Glass or Speculum being rendered perfect, so as that all sorts of Rays, proceeding from one lucid Point in it's Axis, shall be collected by means of the Lens exactly in another Point, it's Aperture may then be extended to it's furthest Limits; and that is, till the whole Pupil of the Eye (or the whole Portion of the Eye-Glass to be used, when that becomes necessarily less than the Pupil) be filled with Rays proceeding from the Speculum, and flowing from one Point of the Object, but no farther; because this is a Limitation made by Nature in the Structure of the Eye itself: And in Telescopes whose Construction is such as we have now described, the largest Aperture of the Speculum that can ever be of Use, will be to the Diameter of the Pupil of the Eye, very nearly, in a *Ratio* compounded of the *Ratio's* of the Focal Length of the Speculum to the Distance of that Focus from the Lens, and of the Distance of the Lens from the Focus of the Telescope, to Unity: That is, of B F to P H, and of R H to 1; which Proportion holds, whatever be the Charge or the Power of Magnifying.

But if Inquiry be made as to the Charge most proper and convenient that will be determined best by Experience, in these, as well as in all other sorts of Telescopes: However, on Supposition that one of a given-Length has it's Aperture and Charge rightly ordered and proportioned, the Rule for preserving the same Degree of Brightness and Distinctness, in all others of a like Construction, will be, to make the Apertures, and magnifying Powers, directly as the Focal Lengths of the Speculums; which shews the vast Advantage and Perfection of these Telescopes, above the common reflecting ones; where, according

* *See* Fig. 48.

to

to Sir *I. Newton*'s Rule, the Apertures, and Powers of Magnifying, must be as the Biquadrate Roots of the Cubes of their Lengths*.

It is likewise a considerable Advantage in this Construction, that the Reflection from the concave Side of the Speculum will do no sensible Prejudice; because the Image of any Object made thereby, is removed to so vast a Distance from the principal Image, formed by the convex Surface, as to create no manner of Confusion or Disturbance in the Vision; which necessarily happens, in some Degree, from the Vicinity of those Images, when the Glass is ground concave on one Side, and as much convex on the other; according to the Method propounded by Sir *I. Newton*, in his *Opticks*.

It may be imagined, perhaps, at first View, that (if our Reasoning is just) the Errors of refracting Telescopes, occasioned by the different Refrangibility of Light, may be corrected by a like Artifice: But the Aberration of the Rays from the principal Focus is there so great, and bears so considerable a Proportion to the Focal Length of the Telescope, that the Error cannot be rectified by the Interposition of any Lens, until the Rays are, by a contrary Refraction, collected again at an infinite Distance, which renders this Expedient quite useless; however there is no need to despair of accomplishing even this, by other Methods: And, by the way, we may observe, if it were worth while to seek a Remedy for the Errors occasioned by the spherical Figure of the Object-Glass only, in Dioptrical Telescopes; that might be obtained by the proper Application of a suitable Lens, between the Focus and the Vertex of the Object-Glass; which is much more easy and practicable, than the grinding of Glasses to Hyperbolical or Elliptical Figures.

For a further Illustration of what is gone before, it may be proper to exhibit the several Parts and Proportions of a Telescope in Numbers computed according to the Theorems already delivered; and in Practice we judge it will be most convenient, that the *Radii* of the Spheres to which the concave and convex Sides of the Speculum are ground, be nearly in the Ratio of 6 to 5; as in the following Example; where A B C D E F, represents the great Speculum of Glass, ground concave on one Side, and convex on the other; quicksilvered over the convex Side, and of an equal Thickness all round it's Circumference. Fig. 48.

The *Radius* of Concavity $= a =$ 48 Inches.
The *Radius* of Convexity $= e =$ 40 Inches.

Then putting n, the Sine of Incidence $= 100$; m, the Sine of Refraction of the least refrangible Rays, out of Glass into Air, $= 154$; and μ, the Sine of Refraction of the most refrangible Rays, $= 156$; as Sir *I. Newton* found them by Experiments; we shall have,

* *See his Opticks, Ed.* 2. p. 97.

P B, the Focal Length of the Speculum with regard to the most refrangible Rays = 18.2926 +, which will be somewhat increased by the Thickness of the Glass, when that is considerable.

P Q, the greatest Aberration of the Rays, occasioned by their different Degrees of Refrangibility, = .05594 +, which Quantity, in Practice, should be a very little augmented, rather than otherwise; wherefore we put it here = .056 = ε.

The *Radius* of the concave Surface of the Lens, turned towards the Speculum, *viz.* of G H I, = υ = 2.8 Inches.

The *Radius* of the concave Surface of the Lens, turned from the Speculum, *viz.* of K L M, = 6.7 Inches.

The Thickness of the Lens at the Vertex L H = $\frac{1}{10}$ of an Inch.

The Aperture of the Lens must be about $\frac{1}{6}$ of the Aperture of the Speculum.

H P, the Distance of the Focal Point P from the Point H, where the abovesaid Lens is to be placed, so as to correct the Errors arising from the different Refrangibility of the Rays, and also the Errors of the spherical Figure, = 2 . $\frac{24}{73}$ Inches.

H R, the Distance of H the Vertex of the Lens from R the Focus of the Telescope, = 6.8 Inches.

And if we suppose the Diameter of the Pupil of the Eye to be $\frac{1}{8}$ of an Inch, (though it has not one certain Measure) then the Diameter of the greatest Aperture of the Speculum, that can ever be of Use, will be 6 $\frac{2}{3}$ Inches, nearly.

The small plano-convex Eye-Glass O must always have one common Focus with the Telescope, to wit, the Point R translated to *r*, by Reflection from the Base of the Prism N; for which Reason it must retain, at all times, an equal and invariable Distance from the Lens G H I K L M; which Distance will be the Focal Length of the said Eye-Glass more H R (= H N + N *r*) the Distance of the Lens from the Focus of the Telescope R.

The Form and Position of the Prism N, and the Contrivance of the other Parts necessary, will be much the same as in the *Newtonian* Telescope.

If the Focal Length of the Eye-Glass be $\frac{1}{4}$ of an Inch, the Telescope will magnify about 200 times.

This Telescope may be contrived in the *Gregorian* way, by using, instead of a Lens and Prism a small Speculum spherically concave on one side, and convex on the other; but we think it not worth while to attempt this Construction, as an Investigation of the Proportion between the two Surfaces necessarily, in this small Speculum, to unite the Rays proceeding from the great one, into one Point, would be intricate, and the Practice also very difficult; by reason that a little Inaccuracy will, in this Case, occasion Errors much more considerable than a like Imperfection in the refracting Lens.

We

We have hitherto ſuppoſed the *Radius* of the Concavity greater than that of the Convexity; as being moſt convenient and uſeful, on ſeveral Accounts, in forming this kind of Teleſcopes; however, it may be proper to remark, that the ſame Method may be uſed for correcting the Errors of the Speculum, when the *Radius* of it's Concavity is leſs than that of the Convexity; only the refracting Superficies of the Lens placed between it's Vertex and Focus, will be convex, and not concave, as in the former Caſe. And there is another thing worthy of Remark, that the Focus, or Point (P), where the moſt refrangible Rays are collected, will fall farther from the Vertex of this Speculum, than the Focus of the leaſt refrangible (Q); a Circumſtance which never happens by Refraction alone, in Glaſſes of any Figure whatſoever, or howſoever they be diſpoſed.

Now all things being put as before, and making H Q = d, I ſay the convex Superficies G H I of a Lens placed at H, that ſhall correct the Errors ariſing from the different Refrangibility of Rays, in this kind of Speculum, will be part of a Sphere, whoſe *Radius* is $= \frac{(\mu - m) \times (d d + d \varepsilon)}{(\mu - m) d + n \varepsilon} = \upsilon$. And H R, the Diſtance of the Point R, where the Rays of all ſorts will unite, after this Refraction, from H the given Point in the Axis, will be $= \frac{\mu d \upsilon}{(\mu - n) d + n \upsilon}$; which Point R being taken as a Centre, deſcribe thereon the Arch K L M, and the Figure G H I M L K will repreſent the Section of a Meniſcus-Glaſs, or Lens, which, placed at the Point H, aſſumed between the Vertex and Focus of the Speculum, will collect all ſorts of Rays proceeding therefrom into one and the ſame Point, or Focus, R. We might alſo ſhew, how this Error may be rectified by one or more Glaſſes, placed in the Axis, at a Diſtance farther from the Vertex than the Focal Point P; but the former Speculum is ſo much preferable to this, for the conſtructing of Teleſcopes, that we think it not worth while to proſecute this Matter farther. To conclude this Eſſay; Fig. 49.

Whoever ſhall think fit to put the Method here propoſed in Execution, we dare venture (from a Trial that has been made) to aſſure him of Succeſs; provided the ſame Diligence, Care, and Accuracy, be applied, in chooſing, figuring, poliſhing, and foiling, the Glaſs, that has of late been employed for the forming Speculums of Metal; and let none be diſcouraged, though the firſt and ſecond Attempt ſhould fail; for that muſt be expected, if the ordinary way of grinding and poliſhing be uſed: Greater Exactneſs is here required, than is uſually thought ſufficient for the Object-Glaſſes of refracting Teleſcopes: Let it be alſo conſidered how many Eſſays, for a long Term of Years, were made by Mr *Gregory*, Sir *I. Newton*, and others, to reduce their Conſtructions of the reflecting Teleſcope into Practice without anſwering, in

in any tolerable Degree, what their Theories promiſed: The Workmen they employed were chiefly Optical Inſtrument-makers, and had it been left to ſuch Perſons only to perform by themſelves, we have reaſon to think, that it would have been pronounced impracticable to this Day, to make a reflecting Teleſcope that ſhould equal or excel refracting ones of ten times it Length; though we now ſee, that moſt of theſe Artificers are capable of making them to ſuch a Degree of Perfection as was formerly deſpaired of. *April* 5. 1739.

A Catoptric Microſcope. By Robert Barker, *M. D. F. R. S,* No. 442. p. 259. *July &c.* 1736.

III. Though *Microſcopes*, compoſed of Refracting Glaſſes only, have been vaſtly improved, as to their Effects of magnifying; yet they have been attended with ſuch great Inconveniences, that their Application to many Arts, in which they might be very convenient, is not ſo common as might be expected, and Mankind have reaped but a ſmall Part of the Advantage obtainable from ſo ſurprizing and uſeful an Inſtrument.

Among the Inconveniences mentioned, theſe are the moſt conſiderable:

1. That in order to magnify greatly, it is neceſſary the Object-Glaſs be a Portion of a very minute Sphere, whoſe *Focus* being very ſhort, the Object muſt be brought exceeding near; it will therefore be ſhaded by the *Microſcope*, and not viſible by any other Light than what paſſes through itſelf; in this Caſe therefore, opake Objects will not be ſeen at all.

2. Objects illuminated this way, may be rather ſaid to eclipſe the Light, than to be truly ſeen, little more being exactly repreſented to the Eye. than the Out-line; the Depreſſions and Elevations within the Out-line; appearing like ſo many Lights and Shades, according to their different Degree of Thickneſs or Tranſparency; though the contrary happens in ordinary Viſion, in which the Lights and Shades are produced by the different Expoſure of the Surface of the Body to the incident Light.

3. Small Parts of large Objects cannot eaſily be applied to the *Microſcope*, without being divided from their Wholes, which in the Caſe of Vivi-ſection defeats the Experiment, the Part dying, and no more Motion being obſerved therein.

4. The *Focus* in the *Dioptrick Microſcope* being ſo very ſhort, is exceeding nice; the leaſt Deviation from it rendring Viſion turbid; therefore a very ſmall Part of an Irregular Object can be ſeen diſtinctly this way.

To remedy theſe Defects I have contrived a Microſcope on the Model of the *Newtonian* Teleſcope, in which I have been greatly aſſiſted by that excellent Workman, Mr *Scarlet, jun.* I ſhall ſay nothing of the Effects of this Inſtrument, excepting that it magnifies from the Diſtance of 9 to 24 Inches.

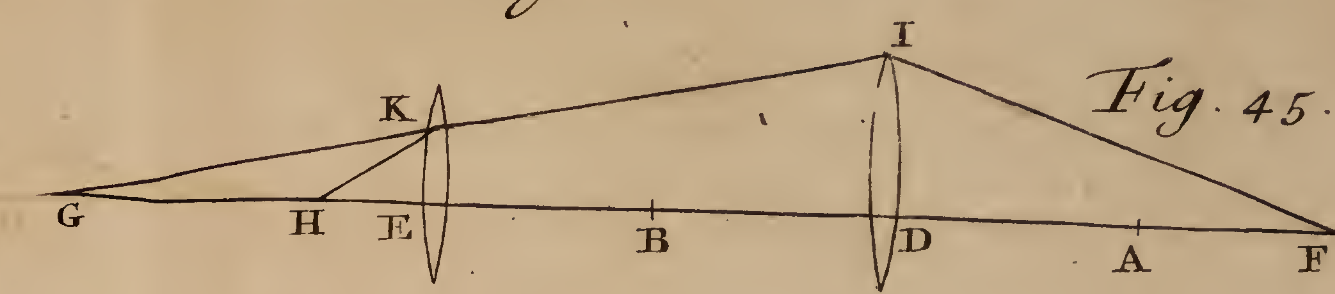


Fig. 45.

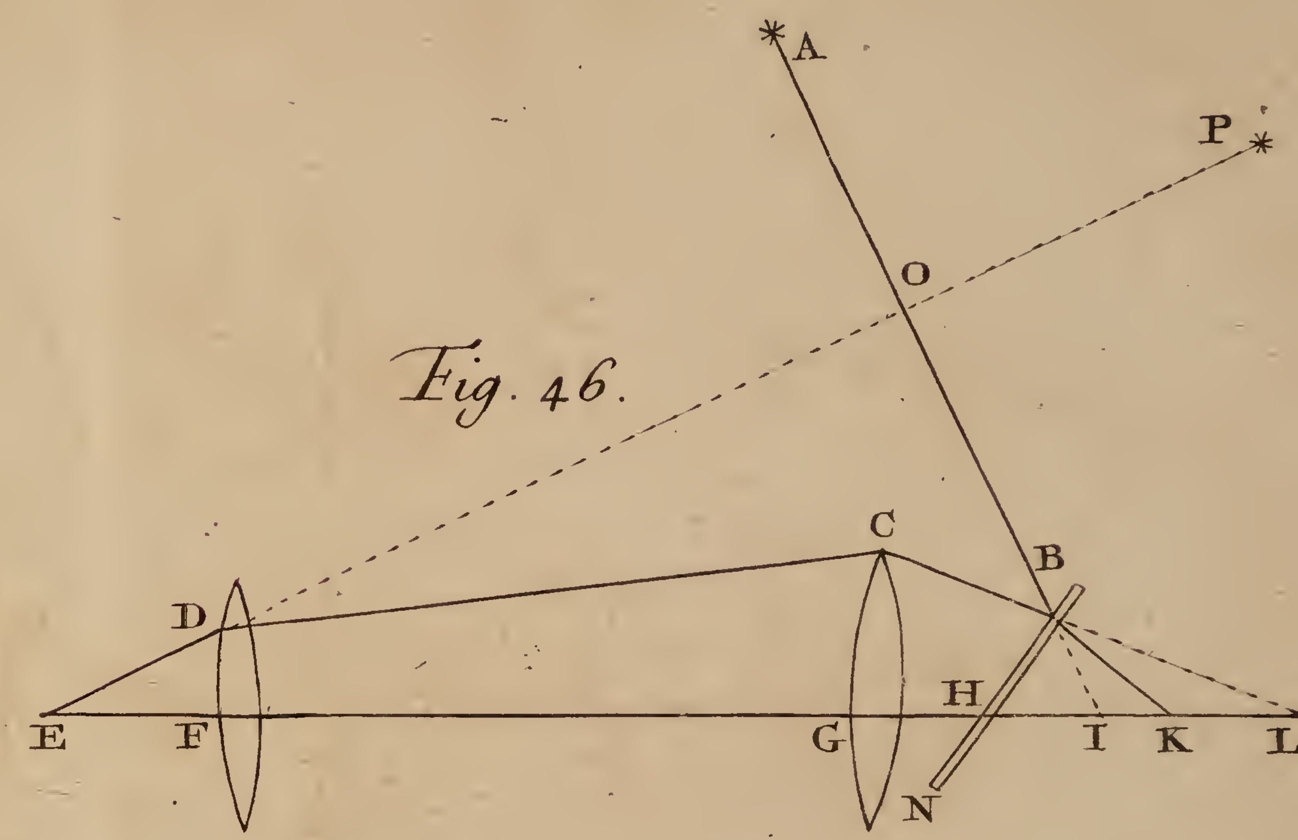


Fig. 46.

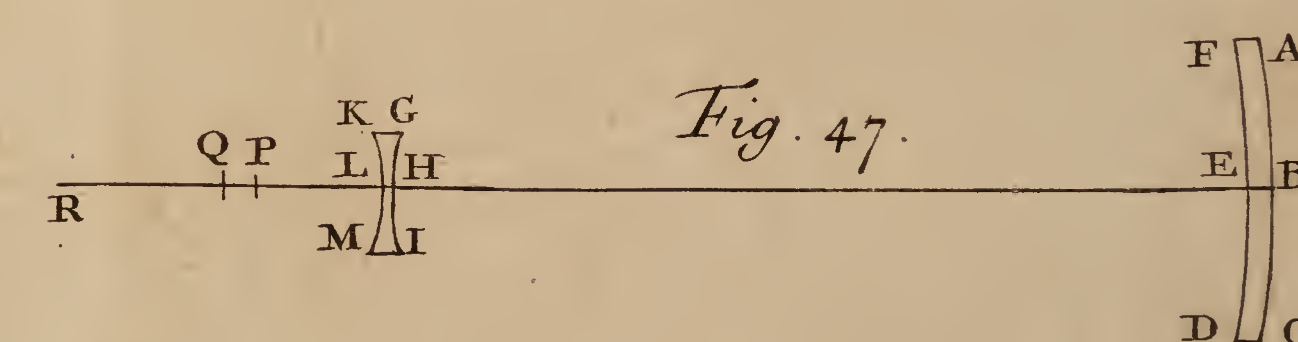


Fig. 47.

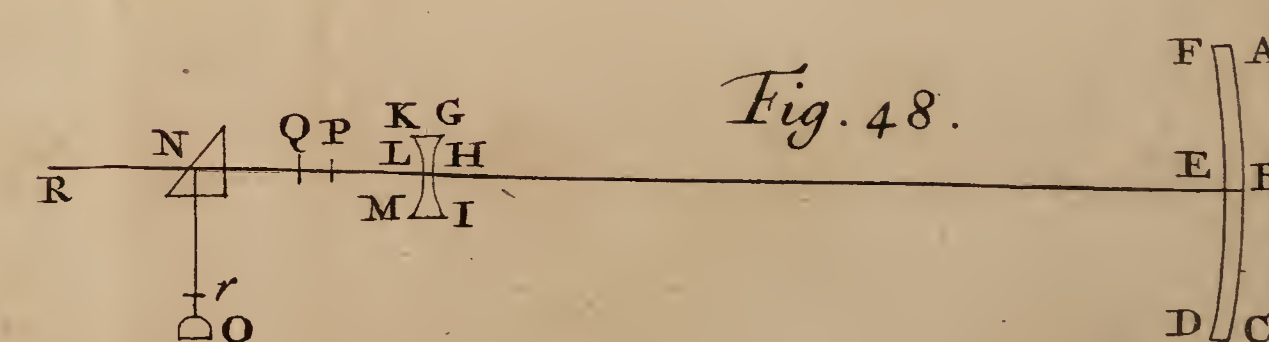


Fig. 48.

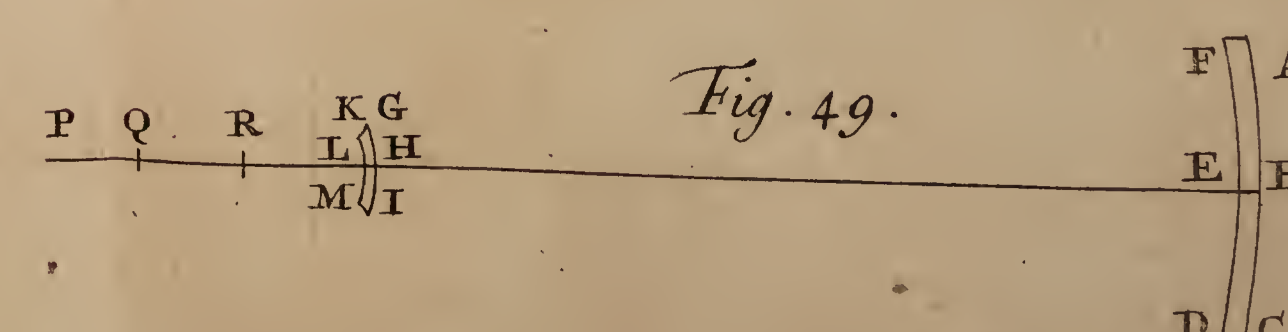


Fig. 49.

Fig. 50. The entire Microſcope mounted on it's Pedeſtal, on a proper Joint, contrived ſo as to direct the Inſtrument towards any Object. *Explanation of the* Figures.

Fig. 51. The Section of the Inſtrument, in which A B is the larger concave metalline *Speculum*, C D is the leſſer concave metalline *Speculum*; E F a hollow Braſs Screw to faſten in the 1ſt Dioptrical Glaſs, or Plano-convex Lens; G H another Screw faſtening on the hollow Cylinder E F I K (in which the Dioptric Glaſſes are contained) to the Body of the Microſcope; I K a Cap with a ſmall Perforation, ſerving as an Aperture to the Eye-Glaſs, or 2d Lens (convex on both Sides); M L is a long Screw paſſing through the Nuts P and V, ſerving to bring the ſmall *Speculum* to a proper Diſtance from the larger; N Q a ſliding Piece moved by the Screw, carrying the Stem Q R, and little *Speculum* C D; Y X a Screw for the Cap at *Fig.* 52; that at *Fig.* 53 is to be ſcrewed on the Aperture I K. Fig. 50, 51. Fig. 52, 53.

Fig. 54. Shews the Conſtruction of the Microſcope, in which *i* is an Object ſuppoſed erect; from which Rays falling on the *Speculum a b*, will be reflected to the *Focus k*, where they will form an inverted Image, and being reflected by the ſmall *Speculum c d*, they will paſs through the Perforation of the great *Speculum*, and falling on the Plano-convex Glaſs *e f*, converge again, and form an erect Image at *l*; which being brought very near to the Eye, and ſo conſiderably magnified, will be diſtinctly ſeen through the Eye-Glaſs *g h*. Fig. 54.

IV. I viewed attentively the Objects applied to theſe Microſcopes by Mr *Leeuwenhoek* himſelf, which Mr *Folkes* * has given a Liſt of in his Account; but the greateſt Part of them were deſtroyed by Time, or ſtruck off by Accident; which, indeed, is no Wonder, as they were only glewed on a Pin's Point, and left quite unguarded. Nine or ten of them, however, are ſtill remaining; which, after cleaning the Glaſſes, appeared extremely plain and diſtinct, and proved the great Skill of Mr *Leeuwenhoek*, in adapting his Objects to ſuch Magnifiers as would ſhew them beſt, as well as in the Contrivance of the Apertures to his Glaſſes, which, when the Object was tranſparent, he made exceeding ſmall, ſince much Light in that Caſe would be prejudicial: But, when the Object itſelf was dark, he enlarged the Aperture, to give it all poſſible Advantage of the Light. The Lens being ſet ſo as to be brought cloſe to the Eye, is alſo of great Uſe, ſince thereby a larger Part of the Object may be ſeen in one View. *An Account of Mr* Leeuwenhoek'*s Microſcopes; by Mr* Henry Baker, *F. R. S.* No. 458. p. 503. Sept. &c. 1740.

It muſt be remembered, that all theſe Microſcopes are of one and the ſame Structure, and that the moſt ſimple poſſible, being only a ſingle Lens, with a moveable Pin before it, on which to fix the Object, and bring it to the Eye at Pleaſure.

Though I was ſenſible it muſt coſt much Trouble to meaſure the focal Diſtances of theſe 26 Microſcopes, and thereby aſcertain their Powers of magnifying, I conſidered that, without ſo doing, it would be im-

* *See* Vol. VI. p. 129.

poſſible to form a right Judgement of them, or make any reaſonable Compariſon between them and our own. This Taſk therefore I have performed, with as much Care and Exactneſs as I was able; and have ſhewn, in the following Table, how many of them have the ſame Focus, and conſequently magnify in the ſame Degree; how many times they magnify the Diameter, and how many times the Superficies of any Objects applied to them. I have given the Calculations in round Numbers, the Fractions making but an inconſiderable Difference; and hope any Miſtakes I may have made in ſo nice a Matter will be excuſed.

A Table *of the* Focal Diſtances *of Mr* Leeuwenhoek's 26 *Microſcopes, calculated by an Inch Scale divided into* 100 *Parts; with a Computation of their magnifying Powers, to an Eye that ſees ſmall Objects at* 8 *Inches, which is the common Standard.*

Microſcopes with the ſame Focus.	Diſtance of the Focus.	Power of magnifying the Diameter of an Object.	Power of magnifying the Superficies.
	Parts of an Inch.	*Times.*	*Times.*
* 1.	$\frac{1}{20}$ or $\frac{5}{100}$	160	25600.
1.	$\frac{6}{100}$	133 nearly.	17689.
1.	$\frac{7}{100}$	114 nearly.	12996.
3.	$\frac{8}{100}$	100	10000.
3.	$\frac{9}{100}$	89 almoſt.	7921 almoſt.
8.	$\frac{1}{10}$	80.	6400.
2.	$\frac{11}{100}$	72 ſomething more.	5184 ſomething more.
3.	$\frac{12}{100}$	66 nearly.	4356 nearly.
2.	$\frac{14}{100}$	57	3249.
1.	$\frac{15}{100}$	53 nearly.	2809 nearly.
1.	$\frac{1}{5}$	40	1600.
26.			

It appears, by the foregoing Table, that one only of theſe 26 Microſcopes is able to magnify the Diameter of an Object 160, and it's Superficies 25600 times; all the reſt falling much ſhort of that Degree. And therefore, I am fully perſuaded, and believe I ſhall be able to prove, that many of the Diſcoveries Mr *Leeuwenhoek* gives an Account of, could not poſſibly be made by Glaſſes that magnify no more than this.

Our Cabinet is but the ſecond in Mr *Leeuwenhoek*'s Collection, and is very far from containing all the Microſcopes he had, as many wrongly

* This largeſt Magnifier of all is in the Box marked 25.

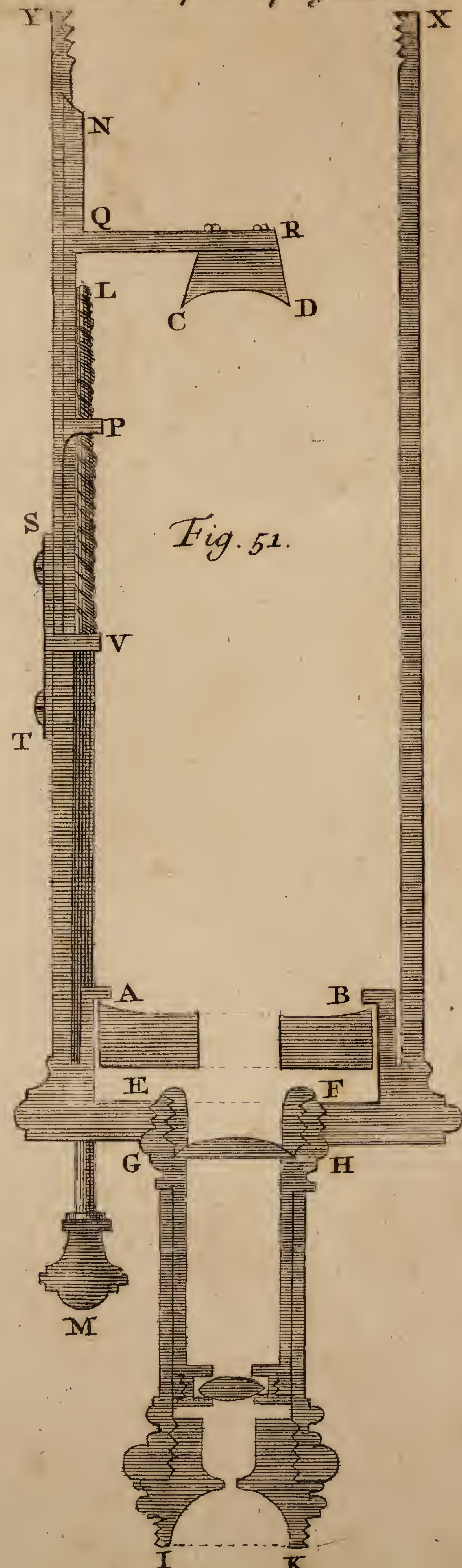


Fig. 51.

Fig. 52.

Fig. 53.

Fig. 50.

Fig. 55.

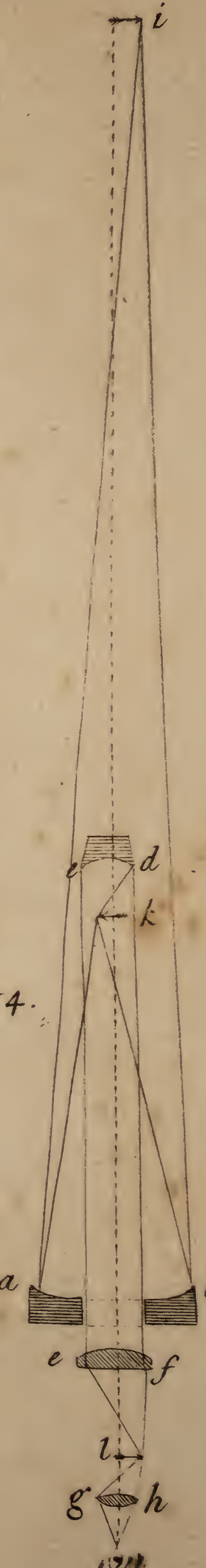


Fig. 54.

have imagined. We find here indeed, 26 Microſcopes in 13 little Boxes: each Box contains a Couple of them, and is marked in two Places with a Number, to diſtinguiſh it from the reſt. But as the firſt of theſe Boxes is marked 15, and the reſt with following Numbers on to 27; it neceſſarily implies there were 14 preceding Boxes, ſince no Man begins with the Number 15. Mr *Leeuwenhoek*, then, had another Cabinet, that held 14 Boxes before ours in numerical Order, and probably each Box contained a Couple of Microſcopes, as our Boxes do. But beſides theſe two Cabinets, he had ſeveral other Microſcopes of different Sorts, as his own Writings will make appear.

Our Cabinet ſeems to have been only his Repoſitory of Objects; for every Microſcope herein was engaged by an Object affixed to it, and thereby rendered uſeleſs for any other Purpoſe; whereas thoſe he employed in his daily Obſervations muſt have been always ready, and at full Liberty, to examine whatever offered. Many of them too muſt certainly have been much greater Magnifiers than any in our Poſſeſſion. And we are aſſured by himſelf, that ſuch he had; for he often mentions his ſhifting Objects from his common to his better, and thence to his moſt exquiſite Microſcopes: And, beſides, (in the ſecond Volume of his Works*) he ſays, "I have an hundred and an hundred Microſcopes, moſt whereof are able to ſhew Objects ſo diſtinctly, even in the cloudyeſt Weather, and by Day-light only, that if the *Animalcula in Semine maſculino* of Animals had the Extremity of their Tails forked, (as deſcribed by a certain Writer) I ſhould eaſily have diſcovered it."——Among this Number, many, without doubt, were contrived for the Examination of Fluids, ſince great Part of his Obſervations were made on them: He informs us alſo, that his Method was to put them into an exceeding ſmall or capillary Tube of Glaſs, which there does not ſeem to be any Means of applyihg to the Microſcopes in our Cabinet, even had they been at Liberty; and much leſs for the larger Tubes he made uſe of to view the Circulation of the Blood in *Frogs*, *Eels*, *Fiſhes*, &c. his Apparatus for which we find in the fourth Volume of his Works†.——But to proceed:

Mr *Leeuwenhoek*, in a Letter to this SOCIETY concerning the *Animalcula* obſerved by him in the *Semen maſculinum* of a *Dog*, which he deſcribes and gives a Draught of, ſays, they were ſo minute, that he believed a Million of them would not equal the Size of one large Grain of Sand ‖. Again, in his 113th Letter, ſpeaking of the *Semen virile*, he declares, that a Million of the *Animalcula* ſeen therein would not equal a large Grain of Sand; and yet he gives a full Deſcription of their Form; for he ſays, their Bodies are roundiſh, ſomewhat flat before, but ending ſharp behind, with Tails exceedingly tranſparent, five or ſix times longer, and about five Times ſlenderer, than their Bodies; ſo that their Figure cannot better be repreſented, than by a ſmall *Earth-nut* with a long Root or Tail.

* Part II. *p.* 290. † Pag. 180. ‖ Vol. I. Part. I. *p.* 160.

Now the Focus of the greatest Magnifier of his being $\frac{1}{20}$ of an Inch, as near as can well be measured, it is capable of magnifying the Diameter of an Object (to an Eye that sees small Objects best at eight Inches) no more than 160, and the Superficies 25600 times: So that Objects, one Million whereof scarce equal a Grain of Sand, viewed through such a Lens, (as only the Superficies can be seen) could appear no larger than $2\frac{1}{2}$ Grains of Sand would be to the naked Eye; and I submit it to be considered, whether that is not too small a Size for any Man to describe so particularly, and delineate the Form and Parts of.

But Mr *Leeuwenhoek* goes yet abundantly farther: For, to mention only one Instance, of which there are several in his Writings; he tells this SOCIETY in his Letter of *July* 25, 1684, that he could discern Vessels in the human Eye, so amazingly minute, that, desiring to know their Smallness, he measured them by the Diameter of a Grain of Sand, (the Process of which Mensuration is there set down) and found by arithmetical Calculation, that a large Grain of Sand must be divided into 18,399,744,000 Parts *, ere it can be small enough to enter these minute Vessels. He must therefore certainly have had Glasses, that were much greater Magnifiers than any we have of his.

It may perhaps be objected, that Mr *Leeuwenhoek* declares, he did not use such small Glasses as some People boasted of; and that, although for 40 Years together he had been possessed of Glasses exceedingly minute, he had employed them very seldom; since, in his Opinion, they could not so well serve to make the first Discoveries of Things, as those of a larger Diameter. In Answer to this, I must observe, that Mr *Leeuwenhoek*, in this Place, is reflecting on a certain Physician, who boasted of an extraordinary Microscope †, scarce bigger than a visible Point, whereby he pretended to discover the *Animalcules in Semine virili* to be exactly of an human Shape, with only a Skin over it. For he says, that while he was attentively observing these *Animalcules*, one of them (a little bigger than the rest) presented itself, having almost slipped off it's Skin: And then there plainly appeared two naked Thighs and Legs, a Breast, and two Arms, above which, the Skin being thrust up, covered the Head as it were a Cap. The Sex he confesses he could not distinguish, and adds, that it died in endeavouring to get clear of the Skin.

Mr *Leeuwenhoek* very justly exposes this romantic Discovery, pretended to be made by this Speck of a Microscope; and takes occasion therefrom to let us know, he does not think such minute Glasses are so much to be depended on as those of a larger Diameter. But there are so many Degrees between the smallest Glass we have of his, (whose Focus is at $\frac{1}{20}$ of an Inch) and this almost invisible Point, that we must not infer from hence he used none of a Size between. Nay, this very Letter seems to imply the contrary; for it tells us, that, in examining

* Vol. I. *p.* 39. † Vol. II. Part. II. Epist. 116. *p.* 84.

the *Semen virile*, he made uſe of 8 or 10 Microſcopes of different magnifying Powers: But as all the Microſcopes we have of his, have Objects faſtened to them, and beſides have no Apparatus for Fluids, I think they could not probably be the ſame he employed for that Examination. May we not rather ſuppoſe he had 8 or 10 different Sizes of Microſcopes, that magnified more than ours? For we know, Fluids require to be examined by the greateſt Magnifiers; and doubtleſs he made uſe of ſuch for that purpoſe.

There is no Advantage in employing a greater Magnifier for any Object, than what is requiſite to ſhew the ſame diſtinctly; but when the Object is exceedingly minute, the magnifying Power of the Glaſs muſt be proportionably great, or elſe it will be impoſſible to ſee the Object clearly. A Lens, (for Example) that ſhews a whole *Flea* diſtinctly, magnifies not near enough to ſhew the *Animalcules* in the *Semen* of that *Flea.*

I am ſenſible, that Mr *Leeuwenhoek*, by long Practice, and uncommon Attention, might be able to diſcern many Objects with theſe Microſcopes, which others, leſs accuſtomed to Obſervations of this kind, cannot readily do: His Eyes too might be ſomewhat different from the Standard I meaſure by. But all theſe Allowances will not, I think, ſuffice to reconcile the Paſſages I have quoted with the Powers of the Glaſſes under Examination.

While I was overlooking theſe Microſcopes of Mr *Leeuwenhoek*, an Opportunity preſented of examining and comparing with them a curious Apparatus of Silver with ſix different Magnifiers, belonging to Mr *Folkes*, and then newly made for him by Mr *Cuff* in *Fleet-ſtreet.* The Body of this Inſtrument, into which the Glaſſes are occaſionally to be faſtened, is after the Faſhion of *Wilſon*'s Pocket-Microſcope, and contrived to ſcrew into the Side of a Scroll fixed on a Pedeſtal, from which a turning *Speculum* reflects the Light upwards upon the Object: It is likewiſe contrived to be uſed with the Apparatus of the Solar Microſcope: Deſcriptions and Figures of both of which I have ſince given in a Book intituled, *The Microſcope made eaſy.* Edit. 2^{d}. *Lond.* 1743. 8vo.

I meaſured the focal Diſtances, and magnifying Powers, of the Six Glaſſes, and found them to be as follows.

A

A Table *of the Six Magnifiers belonging to Mr* Folkes's *Microſcope, calculated by an Inch Scale divided into an hundred Parts, with a Computation of their Powers, to an Eye that ſees Objects at eight Inches.*

Glaſſes.	Diſtance of the Focus.	Magnifies the Diameter.	Magnifies the Superficies.
1ſt.	$\frac{1}{50}$ of an Inch.	400.	160,000.
2d.	$\frac{1}{20}$	160.	25,600.
3d.	$\frac{8}{100}$	100.	10,000.
4th.	$\frac{18}{100}$	44.	1,936.
5th.	$\frac{3}{10}$	26.	676.
6th.	$\frac{1}{2}$	16.	256.

The above Calculation ſhews, that Mr *Folkes*'s Firſt Glaſs magnifies the Superficies of an Object 6 times as much as the greateſt Magnifier of Mr *Leeuwenhoek:* And that the *Animalcula* (a Million whereof, he ſays, ſcarce equalled the Bigneſs of a Grain of Sand) would, if viewed with this Magnifier, appear as large as 16 Grains of Sand do to the naked Eye. And I cannot ſuppoſe but Mr *Leeuwenhoek* had Glaſſes to magnify even more than this, though they are not come to us. For I cannot otherwiſe conceive, how he could obſerve the *Animalcules* in the *Semen maſculinum* of a *Flea*, and of a *Gnat*, as we find he did, or aſſert *, as he does in the ſtrongeſt Terms †, that he could ſee the minuteſt Sort of *Animalcules* in *Pepper-water*, with his Glaſſes, as plainly as he could Swarms of *Flies* or *Gnats* hovering in the Air with his naked Eye, though they were more than ten Millions of Times leſs than a Grain of Sand. And leſt this ſhould be imagined only a random Gueſs, he gives immediately a regular arithmetical Calculation to prove his Computation right. But I believe we muſt all be ſenſible, that no Glaſſes in this Cabinet are able to render ſuch minute Objects diſtinguiſhable.

I am deſirous to do all poſſible Juſtice to theſe Microſcopes, by acknowledging their Excellence, as far as their magnifying Power extends: But I ſhould do wrong to Mr *Leeuwhenhoek*, ſhould I ſuffer the World to believe theſe were his greateſt Magnifiers; ſince whoever hereafter ſhould examine them with that Imagination, would be apt to entertain a bad Opinion of his Veracity.

Experience teaches, that Globules of Glaſs extremely minute, though they magnify prodigiouſly, are ſeldom able to ſhew Objects ſufficiently diſtinct, and therefore are very apt to lead People into Errors: Which

* Vol. IV. *pag.* 21, 22. † Pag. 23.

certainly

certainly was a good Reaſon for Mr *Leeuwenhoek*'s rejecting them: But a ground convex *Lens*, though much ſmaller than any of his before us, if rightly applied, will ſhew exceedingly minute Objects magnified to a ſurpriſing Degree, and with ſufficient Light and Clearneſs, as Mr *Folkes*'s firſt Glaſs witneſſes.

I hope I ſhall not be imagined to intend any Diſreſpect to this famous Man, if I ſuppoſe, that our preſent Microſcopes are much more uſeful and convenient than theſe of his. Let him always be remembered with the higheſt Honour, for the wonderful Diſcoveries he made, and the Microſcopes he has left us, which are indeed extraordinary, when conſidered as the firſt almoſt of their kind: Let us reverence him as our great Maſter in this Art. But the World ſince muſt have been ſtrangely ſtupid, if it could have improved nothing, where there was room for ſo much Improvement. I do not mean as to the Glaſſes (for the Goodneſs of theſe before us, gives juſt Reaſon to believe he might have others as excellent as can perhaps be ever made); but as to the Structure of the Inſtrument they are ſet in, and the Manner of applying Objects to them. And I fancy moſt People will allow, that herein great Improvements have been made: And it is with pleaſure I find, that a large Share of the Credit belongs to our own Countrymen.

One thing alone (which, when ſlightly conſidered, may appear but trifling) has conduced greatly to theſe Improvements; and that is, the making uſe of fine tranſparent *Muſcovy Talc* or *Iſinglaſs*, placed in Sliders, to incloſe Objects in. Had Mr *Leeuwenhoek* known this way, it would have ſaved him a vaſt deal of Expence and Trouble: For then, we may reaſonably ſuppoſe, inſtead of making an entire and ſeparate Microſcope for every Object he was deſirous to keep by him in readineſs to ſhew his Friends, he would probably have ſecured his Objects in Sliders, as we at preſent do, and have contrived ſome ſuch Means as ours, of ſcrewing his ſeveral Glaſſes of different magnifying Powers, occaſionally, to one and the ſame Inſtrument, and of applying his Sliders to which of them he judged beſt. A few good Glaſſes, gradually magnifying one more than other, would, by ſuch a Method, have anſwered all the Purpoſes of his great Number, and his Objects would have been preſerved in a much better Manner.

Two extraordinary Improvements have appeared within theſe two Years, which I beg leave to lay before you, as I think it has not been yet done. I mean, the Solar or *Camera Obſcura* Microſcope, and the Microſcope for opake Objects. Both theſe Inventions we are obliged for to the ingenious Dr *Liberkhun*, who, when he was in *England* laſt Winter was Twelvemonth, ſhewed an Apparatus of his own making, for each of theſe Purpoſes, to ſeveral Gentlemen of this Society, as well as to ſome Opticians, amongſt whom Mr *Cuff*, in *Fleet-ſtreet*, has taken great Pains to improve and bring them to Perfection; and therefore the Apparatus prepared by him is what I am about to deſcribe.

This

This Solar Microfcope is compofed of a Tube, a Looking-Glafs, a convex Lens, and a Microfcope. The Tube is of Brafs, near two Inches in Diameter, fixed in a circular Collar of *Mahogany*, which, turning round at pleafure, in a fquare Frame, may be adjufted eafily to a Hole in the Shutter of a Window, in fuch a manner, that no Light can pafs into the Room but through the aforefaid Tube. Faftened to the Frame by Hinges, on the Side that goes without the Window, is a Looking-Glafs, which, by means of a jointed brafs Wire coming through the Frame, may be either moved vertically or horizontally, to throw the Sun's Rays through the brafs Tube into the darkened Room. The End of the brafs Tube, without the Shutter, has a convex Lens, to collect the Rays, and bring them to a Focus; and on the End within the Room, *Wilfon*'s Pocket-Microfcope is fcrewed, with the Object to be examined applied to it in a Slider. The Sun's Rays being directed by the Looking-Glafs through the Tube upon the Object, the Image or Picture of the Object is thrown diftinctly and beautifully upon a Screen of white Paper, and may be magnified beyond the Imagination of thofe who have not feen it. I affifted lately in making fome Experiments with Dr *Alexander Stuart*, by means of this Inftrument, and a particular Apparatus contrived by him, for viewing the Circulation of the Blood in *Frogs*, *Mice*, &c. and had the Pleafure of beholding the Veins and Arteries in the Mefentery of a *Frog* magnified to near 2 Inches Diameter, with the Globules of the Blood rolling through them as large almoft as *Pepper-corns*. We examined alfo the Structure of the Mufcles of the *Abdomen*, which were prodigioufly magnified, and exhibited a moft delightful Picture.

The Microfcope for opake Objects remedies the Inconvenience of having the dark Side of an Object next the Eye: For by means of a concave Speculum of Silver, highly polifhed, in whofe Centre a magnifying Lens is placed, the Object is fo ftrongly illuminated, that it may be examined with all imaginable Eafe and Pleafure. A convenient Apparatus of this kind, with 4 different Specula, and Magnifiers of different Powers, has lately been brought to Perfection by Mr *Cuff*. Thefe, with the large double reflecting Microfcope, are, I think, the chief, if not the only ufeful Sorts now made in *England*.

I muft not omit taking notice, that Mr *Leeuwenhoek* fays*, that fometimes, to throw a greater Light upon his Objects, he ufed a fmall convex Metal Speculum. How he applied it, I will not pretend to guefs; but it is highly probable our double reflecting Microfcope may be owing to this Hint. I muft alfo obferve farther, that †, after defcribing his Apparatus for viewing *Eels* in Glafs Tubes, Mr *Leeuwenhoek* adds, that he had another Inftrument, whereto he fcrewed a Microfcope fet in Brafs; upon which Microfcope, he tells us, he faftened a little Difh (of Brafs alfo, I fuppofe,) that his Eye might be thereby affifted to fee Objects

* Vol. II. Part. II. *pag.* 93. † Vol. IV. *pag.* 182.

better:

better: For he says, he had filed the Brass which was round his Microscope, as bright as he could, that the Light, while he was viewing Objects, might be reflected from it as much as possible. This Microscope, with it's Dish, (which I give an exact Copy of from the Picture in his Works) seems so like our opake Microscope with it's silver Speculum, that, after considering his own Words, I submit to your better Judgment, whether he is not properly the Inventor of it. His Words are these, —— " *Supra hoc Microscopium Catillum ferruminavi, ut oculus* " *objecta tanto melius videret: nam cuprum circa Microscopium, quantum* " *pote, lima abraseram, ut Lumen in conspicienda objecta, quantum pote,* " *irradiaret.*" Fig. 55.

A true Copy of a Paper found in the Hand Writing of Sir I. Newton, *among the Papers of the late Dr* Halley, *containing a Description of an Instrument for observing the* Moon's Distance *from the* Fixt Stars *at* Sea. *Read* Oct. 28, 1742. No. 465. p. 155.

V. In the annexed Scheme*, *P Q R S* denotes a Plate of Brass, accurately divided in the Limb *D Q*, into $\frac{1}{2}$ Degrees, $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, by a Diagonal Scale; and the $\frac{1}{2}$ Degrees, and $\frac{1}{2}$ Minutes, and $\frac{1}{12}$ Minutes, counted for Degrees, Minutes, and $\frac{1}{6}$ Minutes.

* Fig. 56.

A B, is a Telescope, three or four Feet long, fixt on the Edge of that Brass Plate.

G, is a *Speculum*, fixt on the said Brass Plate perpendicularly, as near as may be to the Object-glass of the Telescope, so as to be inclined 45 Degrees to the Axis of the Telescope, and intercept half the Light which would otherwise come through the Telescope to the Eye.

C D, is a moveable Index, turning about the Centre *C*, and with it's fiducial Edge, shewing the Degrees, Minutes, and $\frac{1}{6}$ Minutes, on the Limb of the Brass Plate *P Q*; the Centre *C*, must be over-against the Middle of the *Speculum G*.

H, is another *Speculum*, parallel to the former, when the fiducial Edge of the Index falls on $00^d\ 00'\ 00''$; so that the same Star may then appear through the Telescope, in one and the same Place, both by the direct Rays and by the reflexed ones; but if the Index be turned, the Star shall appear in two Places, whose Distance is shewed, on the Brass Limb, by the Index.

By this Instrument, the Distance of the Moon from any Fixt Star is thus observed: View the Star through the Perspicil by the direct Light, and the Moon by the Reflext (or on the contrary); and turn the Index till the Star touch the Limb of the Moon, and the Index shall shew upon the Brass Limb of the Instrument, the Distance of the Star from the Limb of the Moon; and though the Instrument shake, by the Motion of your Ship at Sea, yet the Moon and Star will move together, as if they did really touch one another in the Heavens; so that an Observation may be made as exactly at Sea as at Land.

And by the same Instrument, may be observed, exactly, the Altitudes of the Moon and Stars, by bringing them to the Horizon; and thereby the Latitude, and Times of Observations, may be determined more exactly than by the Ways now in use.

In the Time of the Observation, if the Instrument move angularly about the Axis of the Telescope, the Star will move in a Tangent of the

the Moon's Limb, or of the Horizon; but the Obſervation may notwithſtanding be made exactly, by noting when the Line, deſcribed by the Star, is a Tangent to the Moon's Limb, or to the Horizon.

To make the Inſtrument uſeful, the Teleſcope ought to take in a large Angle: And to make the Obſervation true, let the Star touch the Moon's Limb, not on the Outſide of the Limb, but on the Inſide.

An Attempt to explain the Phænomenon *of the* horizontal Moon *appearing bigger, than when elevated many Degrees above the* Horizon: *ſupported by an* Experiment. *By the Rev.* J. T. Deſaguliers, *LL. D.* F. R. S. *Communicated* Jan. 30, 1734-5. No 444. p. 390. Nov. &c. 1736.

VI. 1. This apparent Increaſe of the Moon's Diameter (which a Teleſcope with a Micrometer ſhews to be only apparent) is owing to the following early Prejudice, which we have imbibed from Children.

When we look at the Sky towards the Zenith, we imagine it to be much nearer to us, than when we look at it towards the Horizon; ſo that it does not appear Spherical, according to the vertical Section E F G H I*, but Elliptical, according to the Section *e F g h i*. For this I appeal to every body's Senſe of ſeeing; but not to their Reaſon, which is apt to take off the Prejudice in Perſons that have ſome Knowledge of Aſtronomy. Whereas any other Perſon looking up very high towards the Sky, and then forwards near the Horizon, will (when aſked) ſay, that the Sky over his Head appears much nearer. The Sky thus ſeen, ſtrikes the Eye in the ſame Manner as the long arched Roof of the Iſle of a Cathedral Church, or the Cieling of a long Room.

* Fig. 57.

This being premiſed, let us conſider the Eye at C, upon the Surface of the Earth, and imagine C at the Surface to coincide with K at the Centre; to avoid taking into Conſideration that the Moon is really farther from the Eye when in the Horizon, than when it is ſome Degrees high. Now when the Moon is at G, we conſider it as at *g*, not much farther than G; but when it is at H, we imagine it to be at *h*, almoſt as far again. Therefore, while it ſubtends the ſame Angle as it did before (nearly), we imagine it to be ſo much bigger as the Diſtance ſeems to us to be increaſed.

I have contrived the following Experiment to illuſtrate this:

Fig. 58.

I took two Candles of equal Height and Bigneſs, A B, C D, and having placed A B at the Diſtance of 6 or 8 Feet from the Eye, I placed C D at double that Diſtance; then cauſing any unprejudiced Perſon to look at the Candles, I aſked which was biggeſt? and the Spectator ſaid they were both of a Bigneſs; and that they appeared ſo, becauſe he allowed for the greater Diſtance of C D; and this alſo appeared to him, when he looked thro' a ſmall Hole. Then deſiring him to ſhut his Eyes for a Time, I took away the Candle C D, and placed the Candle E F cloſe by the Candle A B, and though it was as ſhort again as the others, and as little again in Diameter, the Spectator, when he opened his Eyes, thought he ſaw the ſame Candles as before. Whence it is to be concluded, that when an Object is thought to be twice as far from the Eye as it was before, we think it to be twice as big, though it ſubtends but the ſame Angle. And this is the Caſe of the Moon, which appears to us as big again, when we ſuppoſe it as far again, though it ſubtends but the ſame Angle.

The Difference of Diſtance of the Moon in *Perigeo* and *Apogeo*, will account for the different Bigneſs of the horizontal Moon at different Times, adding alſo the Conſideration of the Faintneſs which Vapours ſometimes throw on the Appearance.

2. Having made an Experiment with three Ivory Balls for Confirmation of what I had advanced, that the Deception ariſes from our judging the *horizontal Moon* to be much farther than it is; ſome Gentlemen of the Society were convinced by the Experiment, but others were not; which obliges me to give this further Account of it, that People may judge of the Thing in Writing, which could not be ſo well attended to in the Hurry of ſeveral Perſons viewing the Experiment in Haſte. *An* Explication *of the foregoing* Experiment. *By the ſame.* Ib. p. 392.

1. Two equal Ivory Balls were ſet one beyond another in reſpect of the Eye at E, namely, A B at 20 Feet Diſtance from the Eye, and C D at 40. Fig. 59.

2. It is certain, by the Rules of Optics, that the Eye at E or F will ſee the Ball C D under an Angle but half as big as it ſees the Ball A B; that is, that the Ball C D muſt appear no bigger than the Ball *o* P placed by the Side of A B.

3. But when looking at the two Balls with the naked Eye in an open Room, we conſider that C D is as far again from the Eye as A B, we judge it to be as big as A B, (as it really is) notwithſtanding it ſubtends an Angle but of half the Bigneſs.

4. Now if, unknown to the Spectator, (or while he turns his Back) the Ball C D be taken away, and another Ball *o P* of half the Diameter be placed in the ſame Line, but as near again, at the Side of A B, the Spectator thinking this laſt Ball to be at the Place of C D, muſt judge it to be as big as C D, becauſe it ſubtends the very ſame Angle as C D did before.

It follows therefore, That if a Ball be imagined to be as far again as it really is, we make ſuch an Allowance for that imagined Diſtance, that we judge it to be as big again as it is, notwithſtanding that the Angle under which we ſee it, is no greater, than when we look at it, knowing it's real Diſtance.

For this Reaſon the Moon looks bigger in the Horizon, and near it, than at a conſiderable Height, or at the Zenith: Becauſe it being a common Prejudice to imagine that Part of the Sky much nearer to us which is at the Zenith, than that Part towards the Horizon; when we ſee the Moon at the Horizon, we ſuppoſe it much farther; therefore as it ſubtends the ſame Angle (or nearly the ſame Angle) as when at the Zenith, we imagine it ſo much bigger as we ſuppoſe it's Diſtance greater.

The Reaſon why this Experiment is hard to make, is becauſe the Light from the Ball *o P* is too ſtrongly reflected on account of it's Nearneſs; but if we could give it ſo little Light as to look no brighter than the Ball C D, it would deceive every body. I have made the Experiment ſo as to deceive ſuch as were not very long-ſighted; but I

muſt confeſs I have found it very hard to deceive thoſe who ſee at a great Diſtance; tho' they would all be deceived, if the Diſtances were of 300 or 600 Feet. Now in the Caſe of the Moon, the Deceit is helped, becauſe the Vapours, through which we ſee it when low, take away of it's Brightneſs, and therefore have the ſame Effect as would (or does) happen in the Experiment, when the Light of the Ball *o* P ſtrikes the Eye no ſtronger than the Light of the Ball C D.

CHAP. III.

ASTRONOMY.

Obſervations of the Appearances among the Fixt Stars, called Nebulous Stars, *by* W. Derham, *D. D Canon of* Windſor, *F. R. S.* No. 428. p. 70.

I. THESE Appearances in the Heavens, have borne the Name of *Nebulous Stars*: But neither are they *Stars*, nor ſuch Bodies as emit, or reflect Light, as the Sun, Moon, and Stars do; nor are they *Congeries*, or *Cluſters* of Stars, as the *Milky Way*: but whitiſh *Areæ*, like a Collection of *Miſty Vapours*; whence they have their Name.

There are many of them diſperſed about, in diverſe Parts of the Heavens. There is a Catalogue of them in *Hevelius*'s *Prodromus Aſtronomiæ*, which may be of good uſe to ſuch as are minded to inquire into them.

Beſides theſe Dr *Halley* * hath mentioned one in *Orion's Sword*; another in *Sagittary*; a third in the *Centaur* (never ſeen in *England*) a fourth preceding the right Foot of *Antinous*; a fifth in *Hercules*; and that in *Andromeda*'s *Girdle*.

Five of theſe ſix I have carefully viewed with my excellent eight Foot Reflecting Teleſcope, and find them to be Phænomena much alike; all except that preceding the right Foot of *Antinous*, which is not a *Nebuloſe*, but a *Cluſter* of *Stars*, ſomewhat like that which is in the *Milky-Way*.

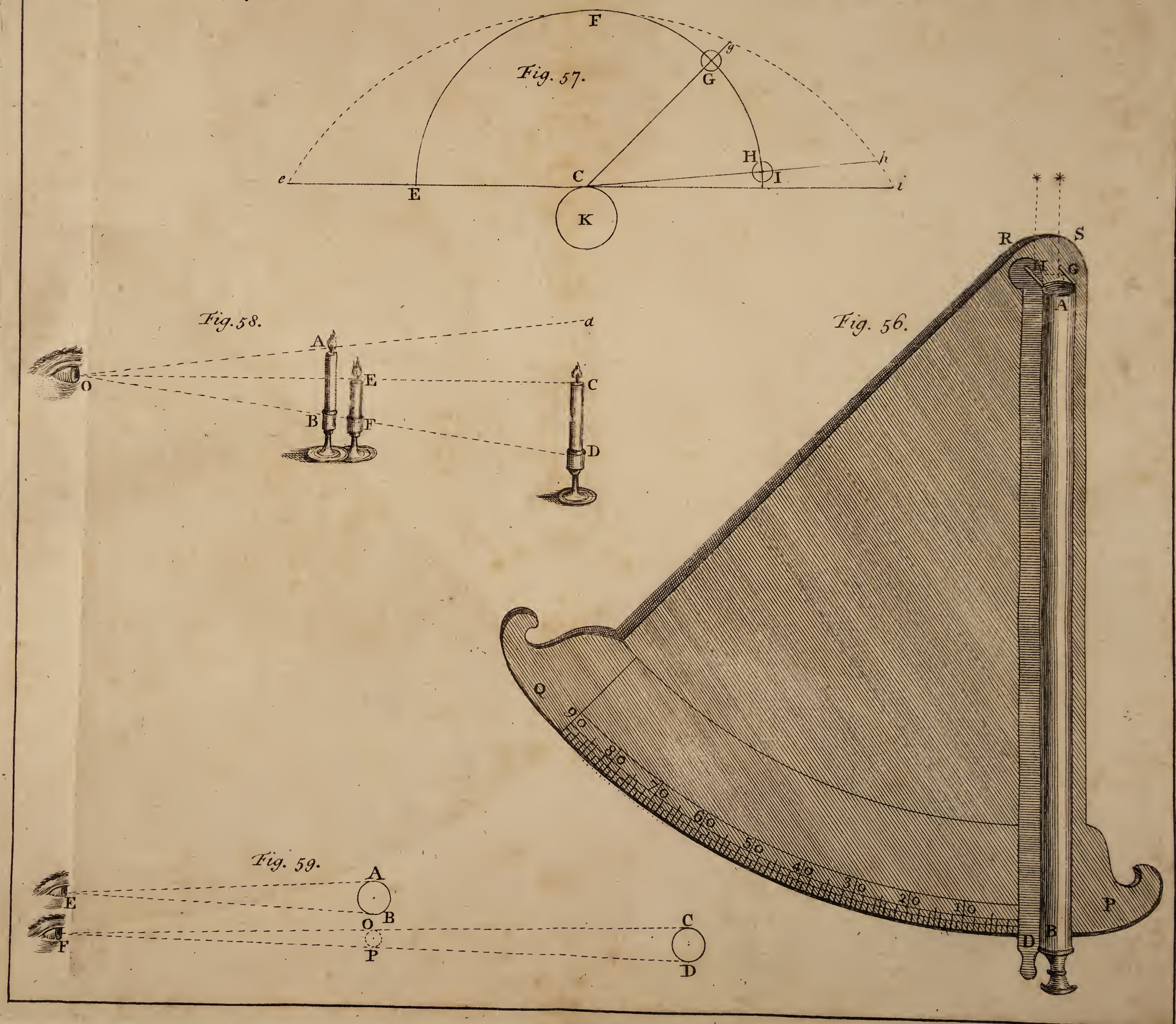
Between the other four, I find no material Difference, only ſome are rounder, ſome of a more oval Form, without any Fixed Stars in them to cauſe their Light; only that in *Orion*, hath ſome Stars in it, viſible only with the Teleſcope, but by no means ſufficient to cauſe the Light of the *Nebuloſæ* there. But by theſe Stars it was, that I firſt perceived the Diſtance of the *Nebuloſæ* to be greater than that of the *Fixed Stars*, and put me upon enquiring into the reſt of them. Every one of which I could very viſibly and plainly diſcern, to be at immenſe Diſtances beyond the Fixed Stars near them, whether viſible to the naked Eye, or Teleſcopick only; yea, they ſeemed to be as far beyond the Fixed Stars, as any of thoſe Stars are from the Earth.

And now from this Relation of what I have obſerved from very good, and frequent Views of the *Nebuloſæ*, I conclude them certainly not to be *Lucid Bodies*, that ſend their Light to us, as the Sun and

* See Vol. I. Chap. iv. §. 13. and Vol. IV. Chap. iii. §. 7.

Moon,

Pl a. IX. *Vol. VIII. part 1st. page 132.*



Moon. Neither are they the *combined Light* of *Clusters* of Stars, like that of the *Milky-Way* : But I take them to be *vast Areæ*, or *Regions of Light*, infallibly *beyond the Fixed Stars, and devoid of them.* I say *Regions*, meaning Spaces of a vast Extent, large enough to appear of such a Size as they do to us, at so great a Distance as they are from us.

And since those Spaces are devoid of Stars, and even that in *Orion* itself, hath it's Stars bearing a very small Proportion to it's *Nebulosæ*, and they are visibly not the Cause of it, I leave it to the great Sagacity and Penetration of this Illustrious Society, to judge whether these *Nebulosæ* are particular Spaces of Light ; or rather, whether they may not, in all probabality, be Chasms, or Openings into an immense Region of Light, beyond the Fixed Stars. Because I find in this Opinion most of the Learned in all Ages (both Philosophers, and I may add Divines too) thus far concurred, that there was a *Region beyond the Stars.* Those that imagined there were *Crystalline*, or *Solid Orbs*, thought a *Cœlum Empyræum* was beyond them and the *Primum Mobile* ; and they that maintained there were no such Orbs, but that the Heavenly Bodies floated in the Æther, imagined that the Starry Region was not the Bounds of the Universe, but that there was a Region beyond that, which they called the *Third Region*, and *Third Heaven.*

To conclude these Remarks, it may be of use to take notice, that in *Hevelius*'s *Nebulosæ*, some seem to be more large and remarkable than others ; but whether they are really so, or no, I confess I have not had an Opportunity to see, except that in *Andromeda*'s *Girdle*, which is as considerable as any I have seen. In his Maps of the Constellations, the most remarkable are the three near the Eye of *Capricorn*; that in *Hercules*'s *Foot* ; that in the third Joint of *Scorpio*'s *Tail* ; and that between *Scorpio*'s *Tail* and the *Bow of Sagittary.* But if any one is desirous to have a good View of these, or any other of the *Nebulosæ*, it is absolutely necessary that he should make use of very good Glasses, else all his Labour would be in vain, as I have found by Experience.

II. 1.

Observation of the Moon's *Transit by* Aldebaran, April 3, 1736, *made at* London *by* John Bevis, *M. D.* No. 446. p. 90. July, *&c.* 1737. Fig. 60.

Apparent Time. H.	M.	S.	
7	40	00	The *Moon*'s Body and *Aldebaran* seen together in the distinct Base of the Telescope.
7	45	52	The *Moon*'s southern Limb running along the parallel Thread, the western Limb came to the horary Thread.
7	49	41	The Glass remaining fixed, and *Aldebaran* running along the parallel Thread, (having the same Declination with the *Moon*'s southerly Limb) came to the Intersection of the Threads.
8	13	04	The *Moon* again running along the Parallel, came to the horary Thread.
8	15	50	*Aldebaran* (the Glass remaining fixed) came to the first oblique Thread at *c*.

H.	M.	S.	
8	15	54½	——— to the hoary Thread at *b*.
8	15	59	——— to the second oblique Thread at *a*.
8	59	54	*Aldebaran* in the Line passing through the Cusps, his nearest Distance from the *Moon*'s Body, being somewhat less that the Length of *Mare Crisium*, or nearly $\frac{1}{10}$ of the *Moon*'s Diameter.

Fig. 61.

An Occultation of Aldebaran, Dec. 23, 1728, Styl. Nov. *observed by* D. Christfried Kirchius, *Astronomer Royal at* Berlin. No. 454. p. 223. July, &c. 1739. Fig. 61.

2.

	Time by a Pendulum Clock. h ′ ″		Parts of the Micrometer, with a Tube of 4 Feet.	Value of the Parts of the Micrometer. ′ ″	Corrected Time. h ′ ″	More correct Time. h ′ ″
1	6. 56. 37	⁎ from the Centre of *M. Sinai*	38	15. 21	6. 24. 37	
2	6. 58. 30	⁎ from the Centre of *M. Sinai*	34	13. 42	26. 30	
3	7. 3. 54	Immersion of the Star			6. 31. 54	6. 32. 00
4	8. 5. 29	Emersion of the Star. Tube 9 Feet			7. 33. 29	7. 33. 33
5	5. 35	Emersion certainly made			7. 33. 35	
6	6. 43	⁎ from the Centre of *M. Sinai*	36	14. 32	34. 43	
7	15. 32	⁎ from the nearest Edge of ☽	8	3. 14	43. 32	
8	20	⁎ from *M. Sinai*	46½	18. 47	48	
9	28	Diameter of the Moon or	74 } 73½	29. 73 29. 41	56	
10	29. 12	*M. Sinai* from the nearest Edge	8	3. 14	7. 57. 12	
11	38	Diameter of ☽. Tube 9 Feet	99½	29. 38	8. 6.	
12	42	Diameter of ☽. Tube 7 Feet	75	30	10	
13	46. 55	⁎ from *M. Sinai*	72	29. 5	14. 55	
14	8. 48	Diameter of the Moon	73½	29. 41	8. 16	

The Situation of the Star, with regard to the lunar Spots was observed in the following Manner.

Before *Obs.* 1. 6^{h}. 20′. I observed the Star in a right Line drawn from the southern Edge of *Insula Macra* (*Posidonius*) through the northern Part of *Pontus Euxinus* (Middle of *Mare Serenitatis*) and *M. Ætna* (*Copernicus*) and a Line from *M. Sinai* to the Star almost touched the Shore of *Sinus Sirbonidis* (*M. Humorum*).

At the Time of *Obs.* 1. the Star was in a right Line, drawn from the greater black Lake (*Plato*) through the eastern Parts of *Insula Cercinna* (from *Kepler* toward the East).

At the Time of *Obs.* 2. the Star was in a Line, continued through the Middle of *Palus Mæotis*, and the Middle of *M. Adriaticum* (through the Middle of *Mare Crisium* and S).

At the Time of the Immersion of the Star, the following right Lines coincided with it, and marked the Place of the Circumference of the Moon, where the Star was hid. 1. From the Shore of *Pontus Euxinus* (*M. Serenitatis*) to the Northward, through *M. Ætna* (*Copernicus*). 2. From the Shore of the *Sinus Apollinis*, through the *Loca Paludosa* (from the Shore of *S. Iridum* through *Kepler*). 3. From *M. Sinai* (*Tycho*) through the southern Shore of *S. Sirbonis* (*M. Humorum*).

The

The Emersion of the Star happened over-against *M. Paropamisi* (*Furnerii*) and in a right Line drawn from the greater black Lake (*Plato*) through *Byzantium* (*Menelaus*), which touched the extreme Bay of *Pontus* (*M. Nectaris*).

At the Time of *Obs.* 7. *M. Porphyrites* (*Aristarchus*) the northern Edge of *L. Thespitis* (*Fracastorius*) and the Star in a right Line.

At the Time of *Obs.* 8. the upper *Lacus Hyperboreus* (*Kermes*) the Middle of *Palus Mæotis* (*Mare Crisium*) and the Star in a right Line.

3. *At* Wittenberg *in* Saxony, *observed by* Jo. Frid. Weidler, *F. R. S.* Ibid. p. 225.

h	′	″	
6.	27.	35	Immersion.
7.	29.	20	Emersion.
1.	1.	45	Duration.

The Observation was made by two Spectators at the same time, with one Telescope of 9 Feet, and another of 4.

The Immersion and Emersion were observed about a Minute sooner by the long Tube than by the short one.

The Appulse of the Star to the eastern Edge of the Moon was about 163° of *Hevelius*'s moveable Scheme of the Full Moon. It emerged about 272° of the same Scheme. Therefore a right Line, joining the Points of Immersion and Emersion, touches the Extremities of *Mare Humorum* and *Nubium*, and passes between *Pitatus* and *Mare Nubium*. *Selenograph.* p. 364.

The Sky was not clear at the time of the Immersion, but thin Clouds almost continually wandered before the Moon and Star; and therefore the Star appeared oblong a great while before the Occultation, through the Vapours of the Atmosphere.

4. *In* Fleet-street, London, *observed with a reflecting Telescope of* 15 *Inches in Length*, *by Mr* G. Graham, *F. R. S.* No. 459, p. 632. Jan. &c. 1741.

	h	′	″
The Occultation at —	5.	27.	6.
Emerged at — —	5.	29.	59.
Duration — —	1.	2.	53.

The Sun's Transit at Noon at $11^h. 59'. 52''$. the Clock gaining of the mean Solar Time about one second in a Day.

III. 1. This Observation was made in *Fleet-street*, *London*, with a Telescope of 10 Feet in Length, fitted with a Micrometer. *An Observation of the Eclipse of the Sun on* May 2, 1733, *in the Afternoon, by Mr* G. Graham, *F. R. S.* No. 429. p. 113. July, &c. 1733.

App. Time.

At 5^h	$44'$	$45''$	It began.
6	25	30	The Cusps were vertical.
6	37	30	The Eclipse was greatest, the lucid Part of the Sun's Diameter measuring 426 Parts, whereof the Sun's Diameter measured 2311. So that the Eclipse was $9\frac{4}{5}$ Digits.
6	46	00	The Cusps were horizontal.
7	28	23	The Eclipse ended.

Of the same by Mr Stephen Gray, *F.R.S.* Ibid. p. 114.

2. I observed the late Eclipse of the Sun, at *Norton*-Court, near *Feversham* in *Kent*, the Seat of *John Godfrey*, Esq; and the Week following, being with *Granville Wheler*, Esq; at *Otterden*-Place, near *Lenham* in *Kent*, he was pleased to communicate to me his Observations of the said Eclipse.

At Norton-Court *by Mr* Gray, p. 115.

		Appar. Time h. m. s.	
Observat.	1	5. 49. 15	Beginning
	2	5. 53. 15	1 Digit
	3	5. 57. 30	2 Digits
	4	6. 2. 55	$3\frac{1}{4}$
	5	6. 11. 50	5
	6	6. 16. 43	6
	7	6. 21. 7	7
	8	6. 27. 0	8
	9	6. 32. 45	9
	10	6. 37. 30	$9\frac{1}{2}$
	11	6. 40. 0	$9\frac{7}{8}$ Greatest
	12	6. 56. 56	8
	13	7. 0. 35	$7\frac{5}{8}$
	14	7. 7. 0	6
	15	7. 11. 55	5
	16	7. 17. 0	4
	17	7. 21. 15	3
	18	7. 25. 55	2
	19	7. 32. 30	End

Our Observations were made with an Helioscope, or Instrument, consisting of a Telescope and Box, with a Digit Scheme at the End of it. The Telescope was 6 Feet, the Box 2 Feet in Length, and the Sun's Image on the Scheme was 6 Inches $\frac{8}{10}$ in Diameter. The Clock was rectified on the Day of the Eclipse, and proved to need no Correction for several Days afterwards, by Observations of the Sun on the Meridian. The Sun's Transit was taken by the Passage of it's Rays through a Hole made in a Brass Plate, the Center of which Hole was at 6 Feet and 3 Inches perpendicular Height, above the horizontal Plane on which the Meridian Line was drawn.

At Otterden-Place, *by Mr* Wheler, p. 116.

Mr *Wheler* observed the Beginning at $5^h\ 49'\ 0''$, and the End at $7^h\ 31'\ 49''$. His Observations were made with a Telescope of 15 Feet in length, and his Time was also rectified by a Meridian Line; but it was done by a Transit of the Rays through a Hole at a much greater Height. For the Brass Plate, in which the Hole was made, was fixed to a Window in the Roof of his Hall, at the Height of 27 Feet above the Meridian Line on the Floor.

3. The

3. The Beginning at $5.^h$ $34.^m$ 00^s $17.^\circ$ $45'$ } Sun's Altitude.
at 6. 00. 00 13. 36
End at 7. 14. 30 2. 45

Of the same, by Mr J. Milner, *at* Yeovil *in* Somersetshire. Ibid. p. 116.

I made use of a Quadrant 2 Feet *Radius.*
Lat. *Yeovil,* 51°.

4. The Latitude of *Gottenburg* is $57^\circ.\ 40'.\ 54''$.

At Gottenburg *in* Sweden, *by* D. Birgerus Vassenius, *Reader of Mathematicks.* Ibid. p. 134.

The Beginning of the Eclipse, which could not be observed because of the Clouds, seems to have happened before 6^h. $26'$. p. m.

h ′ ″	
6. 38. 43	The Sun was about 3 Digits eclipsed.
6. 49. 52	Six Digits, more or less.
7. 14. 6	♃ appeared.
7. 14. 46	The whole Disk of the Sun began to be covered.
7. 15. 50.	The greatest Darkness, when all the Stars of the Great Bear, the Lion's Heart, Sirius, Procyon, the Bull's Eye, and some others were visible: but neither ☿ nor ♂ were seen.
7. 16. 54	The Sun began to dart his Rays with incredible Quickness.
7. 20. 12	♃ still appeared.
7. 41. 38	The Sun was 6 Digits eclipsed.
8. 5. 50	The End of the Eclipse, the whole Disk of the Sun shining. Total Duration of the Eclipse at *Gottenburg,* $2'\ 8''$.

The total Duration of this Eclipse in a Place called *Swenaker,* 7 *Swedish* Miles from hence, in Latitude $58^\circ\ 15'$, was, according to the Observation of my Brother *Torstanus Vassenius* by a Pendulum, $2'\ 31''$.

Whilst the Sun was totally covered, I saw not only the greatest Part of the Spots in his Disk, but also the Atmosphere of the Moon, with a Telescope of about 21 *Swedish* Feet; it was a little brighter at the western Limb of the Moon, at the time of the greatest Immersion; but without that Irregularity and Inequality of the luminous Rays, which appeared to those who looked without a Telescope. But the most worthy of Observation were 3 or 4 little reddish Spots in it, seen without the Circumference of the lunar Disk; one of which was greater than the rest, about the middle Way between the South and West, according to the best Judgment that could be made. This was composed of 3 smaller parallel Parts or *Nubeculæ,* of unequal Length, with some Obliquity to the Circumference of the Moon. I saw it plainly preserve the same Situation for $40''$ or more: but at length a Ray of the Sun breaking out like Lightning deprived me of any farther Opportunity of observing so beautiful a *Phænomenon.*

At Wittemberg *in* Saxony, *by* Joh. Frid. Weidler, *Prof. Math. and* F. R. S. No. 433. p. 332. July, *&c.* 1734.

5. Phases of the beginning Eclipse.

h	′	″		h	′	″	
6	36	5	p. m. Beginning of the Eclipse	7	2	50	six Digits
	39	50	one Digit		7	50	seven Digits
	45	1	two Digits		10	50	eight Digits
	48	50	three Digits		15	50	nine Digits
	52	50	four Digits		19	50	ten Digits
	58	5	five Digits		29	20	eleven Digits

Some light Clouds come over the Sun.

Phases of the decreasing Eclipse.

h	′	″		h	′	″	
7	35	50	ten Digits	7	44	50	eight Digits
	40	50	nine Digits		46	5	the Sun sets

Fig. 62.

The Circle drawn in the Figure represents the Image of the Sun, of the same Magnitude as it appears at the Bottom of the Helioscope.

The Light of the Sun near the Orb of the Moon, which I have usually observed, in other Solar Eclipses, to have a vehement Motion and Undulation, was in this Eclipse perfectly still and quiet.

The Orb of the Moon discovered a manifest Asperity to all the Observers, especially in the western Part, in the Phases that were observed a little before the Setting of the Sun; but there were some Intervals, in which the Tops of the Lunar Mountains were distinguished, but not very broad or deep. By the Application of a Scale nicely divided, I estimated the Depth of one Valley to be $\frac{1}{200}$ Part of the Diameter of the Moon.

The last decreasing Phases were seen thro' thin Clouds, and yet the Moon did not hide from us above 11 Inches of the Disk of the Sun.

The Setting of the Centre of the Sun was then found by Calculation to be $7^h\ 39'\ 49''$ for the Horizon of *Wittemberg*, and so it was retarded near 6 Minutes by the Refraction of the Rays in the Clouds of the Horizon.

Eclipse of the Sun April 22, 1734, *observed at* Rome *by the Abbot* Didacus de Revillas, *F. R. S. and* Andreas Celsius, *F. R. S.* Prof. Astron. Upsal. No. 442. p. 296. July, &c. 1736.

IV. This Eclipse was observed with a very good Telescope, of about 6 Roman Palms in Length.

True Time p. m.

h	′	″	Digits.	
22	22	35	0	The Beginning seemed to be a small Matter over thro' the Cloud.
	27	1	$0\frac{1}{2}$	
	34	0	1	
	42	6	$1\frac{1}{2}$	
23	0	52	2	
	3	16	2	Or a little more, and the greatest Darkness seemed to be at Hand.
	10	31	2	
	28	16	$1\frac{1}{2}$	
	45	11	$0\frac{1}{2}$	
	52	1	0	The End.

From

From the 4th and 8th Obſervations we may gather, that the greateſt Darkneſs was about $23^h\ 5'$.

V.

Eclipſe of the the Sun Sept. 23, 1736. *obſerved at* London, *by* J. Bevis, *M.D.* No. 446. p. 98. July, &c. 1737. Fig. 63.

Apparent Time. p. m.

h	'	''	
4	12	55	The northern Limb of the Sun running over the Parallel Thread P P, the weſtern Limb touches the horary Thread H H.
	12	42	The ſmall Spot near the northern Limb reaches the firſt oblique Thread 1.
	13	1	The Spot reaches the horary Thread H H.
	13	20	The Spot reaches the ſecond oblique Thread 2.
	14	45	The eaſtern Limb of the Sun reaches the horary Thread. Then cloudy.
4	45	41	The Sun getting out of the Clouds, the Eclipſe appears thro' the Teleſcope to be but juſt begun.
	45	48	Still imperceptible to the Eye thro' a coloured Glaſs.
	46	00	Now very ſenſible. Then cloudy.
5	5	29	The ſouthern Limb running along the Parallel, the weſtern Limb reaches the horary Thread.
	5	41	The weſtern Cuſp of the Sun reaches the horary Thread.
	7	5	The eaſtern Cuſp touches the horary Thread. Then the Sun was covered with Clouds till it ſet.

Fig. 64.

I place the Beginning at $4^h\ 45'\ 31''$, p. m.

VI. 1.

Eclipſe of the Sun, Feb. 18, 1736-7. *obſerved in* Fleetſtreet, London, *by Mr.* Geo. Graham, *F. R. S.* No. 447. p. 175. Jan. &c. 1738.

Appar. Time. p. m. At

h	'	''		
2	25	9	A ſmall Impreſſion appeared on the Sun's Limb; I judge the Beginning to have been about 5 or 6'' ſooner.	
3	21	28	The middle of the firſt and larger Spot was covered.	
	29	30	The middle of the ſmaller Spot.	
	40	4	The Cuſps perpendicular.	
4	3	34	The Cuſps horizontal.	
	35	32	The middle of the larger Spot emerged.	
	38	21	The ſmaller emerged, or a little before.	
4	52	57	The Chord between the Cuſps — —	1057
	55	00	The Chord — — — —	954
	56	32	The Chord — — —	851
	59	34	The Chord — — —	632

Then a Cloud cover'd the upper Limb, and prevented a Sight of the ending, which was ſoon after.

Between twelve and one a Clock, I meaſured the Diameter of the Sun with a Micrometer. At the Time of the greateſt Obſcuration, the lucid Part of the Sun's Diameter was equal to 392 ſuch Parts as his whole Diameter contained 2188.

I had a Tranſit of the Sun at Noon, and of *Sirius* at Night, which, compared with preceding ones, I found my Clock went too faſt for mean Solar Time, about 1'' in a Day.

——*At the Royal Obſervatory at* Greenwich, *obſerved by* J. Bevis, *M. D. in Company with Dr* Halley, Ibid. p. 176.

2.

Appar. Time. p. m.			At
h	'	''	
2	25	39	The Beginning.
5	3	29	The End.

At the End, the Sun's Limb appeared ſomewhat tremulous, and a ſmall thin Cloud came over it. Dr *Bevis* judged the Time might be relied on to 2 or 3''.

—Edinburgh, *by* Colin Mac Laurin, *Prof. Math. F.R.S.* Ibid. p. 177.

3. In the Hiſtory of Eclipſes collected by *Ricciolus*, there are very few ſaid to be Annular; and of theſe ſome have been controverted, as that ſeen by *Clavius* at *Rome*, *April* 9, 1567, and that ſeen by *Jeſſenius* at *Torgaw* in *Miſnia*, *Feb.* 25, 1598, which are both diſputed by *Kepler*. Some Aſtronomers, Antient and Modern, have been of Opinion, that no Eclipſe can be Annular: and ſince ſuch ſeem to have been rarely obſerved, and I have not met with a particular Deſcription of any of them, I ſhall give as full an Account of this Eclipſe as I can collect from the Obſervations that were made here, and thoſe that have been communicated to me from the Country.

The Sky was generally favourable in the Southern Parts of *Scotland* during the Eclipſe; and though there were great Showers of Snow in the North, they had ſometimes a View of it. There was ſomething very entertaining in the annular Appearance, a *Phænomenon* that was equally new to all who ſaw it, that gave great Delight to the Curious, without ſtriking Terror into the Vulgar. It extended Southward almoſt to *Morpeth* in *Northumberland*, and beyond *Inverneſs* Northward; ſo that a Part of *England*, and almoſt all *Scotland*, were within it's Limits. I have not as yet learned how far the North Limit was from us; but I am informed, that the Weather was very unfavourable there.

Ten Days before the Eclipſe, I wrote to many of my Acquaintance in the Country, deſiring that they would determine the Duration of the annular Appearance as exactly as poſſible; in Hopes, by comparing their Obſervations, to have traced the Path of the Centre and the Limits of this *Phænomenon* after the Example given in 1715, by Dr *Halley*, to whom we owe the beſt Deſcription of an Eclipſe that Aſtronomical Hiſtory affords. I ſhall give an Abſtract of the Accounts I received in Anſwer to theſe Letters, after I have deſcribed our Obſervations at *Edinburgh*.

The Times of the Apearances here were determined by a Pendulum Clock, which Mr *Graham* gave me ſome Years ago, from whom I alſo had the meridian Inſtrument by which it is examined. The meridian Line was often adjuſted in the uſual Manner, and an exact Account of the Sun's Tranſits in the Meridian, and of the Tranſits of

Procyon

Procyon in a fixed Telescope, was kept by Mr *Short* for a long Time before and after the Eclipse; and, by comparing his Observations, I cannot doubt but that the Times were determined with sufficient Exactness. I was often with him when he examined the Meridian, and observed those Transits; particularly the Day of the Eclipse, when by the Sun's Passage in the Meridian, we found that the Clock was before the apparent Time 13 Minutes 27 Seconds; and so much I have subducted from the Times that were marked during the Observation. The Latitude of this Place is commonly said to be 55 Degrees 55 Minutes; and by some Trials we have made lately, this must be near the Truth, though in some Maps and Tables it be represented greater. By comparing an Observation we had here of the End of the Eclipse of the Moon, *Nov.* 20, 1732. with an Observation of the End of the same Eclipse by Mr *Graham* in *Fleetstreet*, the Longitude of this Place is a little more than 12′ of Time further West.

Some Days before the Eclipse, Lord *Aberdour* set up a Clock in the Castle, and adjusted it with mine by a Watch that shewed the Seconds. The Clocks were compared together the Day of the Eclipse at Noon, by a Cannon fired from the Castle, some Persons being appointed to attend each Clock, and mark the Seconds when they heard the Sound: An Allowance of 2″½ being made for the Progress of the Sound, (which was determined by several Trials at Night) the Clock in the Castle was found to be before the apparent Time 12′ 19″, and so much is subducted from the Times that were marked in the Castle during the Observation. It was agreed that we should give Signals to one another mutually at the Beginning and End of the Eclipse, and at the Beginning and End of the annular Appearance. His Lordship's Signal from the Castle was a Cannon, ours from the College a Musquet, Persons being appointed to mark our Signals from a proper Place of the Castle: There is no Regard however had to those Signals in marking the Times of the Appearances. Lord *Aberdour* made use of a reflecting Telescope of 15½ Inches focal Distance, that magnified 90 times; only he observed the annular Appearance with one of 5½ Inches, that he might have a View of the whole Disk of the Sun at once. Mr *Short* observed the Beginning of the Eclipse with a Telescope of 15½ Inches focal Distance, that magnified 104 Times, but the annular Appearance with one of the same Length, that also took in the whole Disk of the Sun, and magnified 50 times. The reflecting Telescope with which I observed the Eclipse from the Beginning to the End, took in the whole Disk of the Sun, (having been made by Mr *Short* for this Purpose) though the focal Distance of the big Speculum be 9½ Inches; and though it bears a higher Charge, I made Use of an Eye-glass on this Occasion, that magnifies only 50 times.

By a Computation that had been made here from Sir *I. Newton*'s Theory, I expected that the Eclipse would begin at 2^h 6′, apparent Time; we therefore looked attentively towards the South-west Part of

of the Sun's Limb from Two o'Clock. At $2^h\ 5'\ 36''$ we perceived a Depreſſion that was juſt diſcernible on the Sun's Limb near that Place; our Signal was then made, but by an Accident Lord *Aberdour* had been hindred from obſerving the Sun at that Time: However, when he looked for it, he ſaw it was begun, and his Signal gave general Intimation of this to the Town, about $40''$ after we had firſt perceived it; and, as far as I have learned, it was not diſcerned by the Eye, though aſſiſted with a ſmoaked Glaſs, till about this Time.

I obſerved the Progreſs of the Eclipſe by a Helioſcope; but after 10 Digits were eclipſed, I returned to the Teleſcope, to attend the Beginning of the annular Appearance. A little before the *Annulus* was complete, a remarkable Point or Speck of pale Light appeared near the Middle of the Part of the Moon's Circumference, that was not yet come upon the Diſk of the Sun; and a Gleam of Light more faint than this Point, ſeemed to be extended from it to each Horn: I did not mark the preciſe Time when I firſt perceived this Light, but am ſatisfied that it could hardly be leſs than $\frac{1}{4}$ of a Minute before the annular Appearance began. Mr *Short* (who was in another Chamber at ſome Diſtance, and made uſe of a larger Teleſcope) aſſures me that he ſaw it $20''$ before the *Annulus* was completed; and this is confirmed by a Call that was then heard from the Chamber where he was, of which I did not underſtand the Meaning till we met afterwards, and upon which the Perſon who made our Signals was about to fire, if I had not forbid him. I was ſurprized with this Light at firſt, and did not immediately recollect that it proceeded probably from the ſame Crown that was ſeen about the Moon in a total Eclipſe of the Sun at *Naples* in 1605; and was obſerved by many in different Parts of *Europe*, in the three late total Eclipſes of 1706, 1715, and 1724. I did not expect to have ſeen this Light, when ſo much of the Sun's Diſk was uncovered; but as I kept only ſo much of the Diſk in the Teleſcope as was neceſſary for aſcertaining the Time of the Formation of the *Annulus*, this muſt have contributed to my diſcovering it; for this Light was very faint, compared with that which appeared upon the Sun's Arch near the ſame Place the Moment it was uncovered, and the *Annulus* completed.

Moſt of thoſe who obſerved the Eclipſe with Teleſcopes, mention in their Letters, that as the *Annulus* was forming, they perceived the Light to break in ſeveral irregular Spots near the Point of Contact, and that the Limb of the Moon ſeemed to be indented there. Some expreſs themſelves as if thoſe irregular Parts had appeared to them in a kind of Motion. It is thus deſcribed by Mr *Bayne*, Profeſſor of the Municipal Law, ' What appeared to me moſt entertaining, con-' ſidered as an Object of Sight, was, when the Extremities of the ' Horns formed upon the Face of the Sun ſeemed as if they had been ' in the Action of uniting their Points, the Inequalities on the ' Extremity of the Moon's Diſk gave the Appearance, as it were,

' of

'of ſmall Bodies in particular Motion.' There was not any Undulation at this Time on the Circumference of the Sun. I find that ſuch Appearances of a tremulous Motion in certain Periods of ſolar Eclipſes are mentioned by *Hevelius* and others. Lord *Aberdour* obſerved the Beginning of the annular Appearance with a ſmaller Teleſcope, and perceived only a narrow Streak of a duſky red Light to colour the dark Edge of the Moon, immediately before the Ring was completed, and after it was diſſolved.

At $3^h\ 25'\ 55''$ the Circumference of the Sun appeared complete, and perfectly circular. We called at the ſame Inſtant to the Perſon who was appointed to make our Signal, and in a Second or two the Cannon from the Caſtle was heard. The *Annulus* appeared to the Eye to be central for ſome time, but in the Teleſcope it was always broader toward the South-eaſt than towards the North-weſt Part of the Sun's Diſk. The Breadth appeared much greater to the naked Eye, than could have been expected from the Difference of the Semidiameters of the Sun and Moon. This was ſo remarkable, that ſuch a *Phænomenon* muſt have confirmed thoſe Aſtronomers in their Opinion, who imagined that the Diameter of the Moon is contracted in her Conjunctions with the Sun. This Appearance proceeded chiefly, I ſuppoſe, from the Light's incroaching on the Shade, as is uſual; but whatever was the Cauſe, every Body ſeemed ſurprized that the Moon appeared ſo ſmall upon the Diſk of the Sun.

It was obſerved, that the Motion of the Moon appeared more quick in the Formation and Diſſolution of the *Annulus*, than during it's Continuance. This is particularly deſcribed by Mr *Fullarton*, of *Fullarton*, in a very exact Account of the Eclipſe, as it appeared at his Seat at *Crosby*, near *Aire*, on the Weſt Coaſt of *Scotland*. He writes that, 'the *Annulus* appeared to be nearly of an uniform Breadth, during the 'greater Part of the Time of it's Continuance, but ſeemed to go off 'very ſuddenly; ſo that when the Diſk of the Moon approached to 'the concave Line of the Sun's Diſk, they ſeemed to run together 'like two contiguous Drops of Water on a Table when they touch 'one another;' and he adds, that it came on in the ſame way. This Appearance ſeems to be accountable from the ſame optical Deception as the former.

During the Appearance of the *Annulus*, the direct Light of the Sun was ſtill very conſiderable; but the Places that were ſhaded from his Light appeared gloomy. There was a Duſk in the Atmoſphere, eſpecially towards the North and Eaſt. In thoſe Chambers that had not their Lights Weſtwards, the Obſcurity was conſiderable. *Venus* appeared plainly, and continued viſible long after the *Annulus* was diſſolved, and I am told that other Stars were ſeen by ſome: One Gentleman is poſitive, that being ſhaded from the Sun, he diſcerned ſome Stars Northwards, which he thinks by their Poſition were in *Urſa Major*.

It was very cold at this Time; a little thin Snow fell; and some little Pools of Water in the College Area, where there was no Ice at two o'Clock, were frozen at Four. A reflecting Telescope of a large Size, and of a much greater Aperture than ordinary, that took in the whole Sun, and burned Cloth very suddenly through the tinged Glass at the Beginning of the Eclipse, and on that Account could not then be used with Safety, was that by which Mr *Short* observed the annular Appearance. Some curious Gentlemen found, that a common Burning-glass, which kindled Tinder at $3^h\ 59'$ and burned Cloth at $4^h\ 8'$ had no Effect during the annular Appearance, and for some time before and after it.

I have mentioned those Things mostly upon the Report of others; for during the greater Part of this Appearance I was observing the Progress of the Moon upon the Disk of the Sun through the Telescope. The first internal Contact of the Disks, at the Formation of the *Annulus*, was considerably below the West Point of the Sun's Disk; and the second Contact, at the Dissolution of the *Annulus*, seemed to be about 10 Degrees Eastwards from the North Point or Zenith of the Disk: But I did not find that the Position of those Points of Contact could be estimated with Exactness on several Accounts. The Breadth of the *Annulus* towards the South-east Part of the Sun's Disk, was at least double of it's Breadth towards the opposite Part, about the Middle of this Appearance. An Apparatus, by which I was in Hopes of being able to determine those Things more accurately, was not ready. I proposed to have made some Estimation of the *Ratio* of the Continuance of the annular Appearance, where it was central to it's Continuance at *Edinburgh*, from that of the Arithmetical Mean betwixt the Numbers that should express the Proportion of the greatest and least Breadth of the *Annulus* to the Geometrical Mean betwixt the same Numbers; or from the *Ratio* of the *Radius* to the Sine of half the Arch intercepted between the two Points of internal Contact; but I did not obtain these *Ratio's* with sufficient Exactness.

At $3^h\ 31'\ 43''$ the *Annulus* was dissolved, after having continued $5'\ 48''$. And here again our Signals were heard immediately after one another: The Middle of the Eclipse was therefore at $3^h\ 28'\ 49''$. In this the Time by Observation did not agree so well with the Time by Computation as in the Beginning of the Eclipse, the Difference being here about four Minutes. The Irregularities of the Moon's Surface occasioned the same Appearances, in some measure, as at the Formation of the *Annulus*. When I returned to the Helioscope, there was some Time lost in directing it towards the Sun; and when I got the Image in a due Position, there was less than 11 Digits eclipsed; and I suspect that it never amounted to full 11 Digits. I had no Micrometer.

After taking some more Digits, I went with Sir *John Clerk* to a neighbouring House, to observe the End of the Eclipse, being afraid

afraid we ſhould not be able to ſee it from the College. By a Signal that was made to the Perſon who attended the Clock, (2″ being ſubducted, that were loſt in making the Signal) the End was at $4^h\ 44'\ 51''$. The Wind blew hard at this Time, ſo that the Teleſcope could not be kept very ſteady, and there was ſome Undulation on the Circumference of the Sun; but I cannot think that the Error of this Obſervation can exceed 3 or 4″, the Circumference of the Sun appearing to me complete at that Inſtant.

I ſhall now ſubjoin the Obſervations that were made in the Caſtle and College in one View, by which you will ſee that they agree preciſely as to the Continuance of the annular Appearance, a Coincidence that could not have been expected; but ſo it is, according to the Numbers that were given me immediately after the Eclipſe by thoſe whoa ttended the Clocks.

	In the College.			In the Caſtle.		
	h	′	″	h	′	″
The Beginning of the Eclipſe at	2	5	36			
The Beginning of the annular Apppearance	3	25	55	3	25	53
The End of the annular Appearance	3	31	43	3	31	41
The End of the Eclipſe	4	44	51	4	44	48

By Lord *Aberdour*'s Obſervations, the lowermoſt and biggeſt of the two Spots that appeared upon the Diſk of the Sun in the upper Part, was touched by the Moon at $3^h\ 4'\ 40''$ and this Spot was wholly covered at $3^h\ 5'\ 19''$. Mr *Short* obſerved another Spot at the Circumference of the Moon, at $2^h\ 24'\ 51''$. Though the Obſervations of the Digits could not be made with ſo much Exactneſs as the preceding, on ſeveral Accounts, I ſhall ſubjoin ſome of them.

		h	′	″
The Sun was eclipſed	2 Digits at	2	21	14
	6 Dig.	2	50	54
After the annular Appearance	9 Dig.	3	45	57
	8 Dig.	3	52	55
	7 Dig.	3	59	53
	6 Dig.	4	6	51

At *Hopeton-Houſe*, nine Miles Weſt, and a little Northwards from *Edinburgh*, Lord *Hope* obſerved the annular Appearance begin at $3^h\ 25'$ the End of this Appearance at $3^h\ 31'$ and the End of the Eclipſe at $4^h\ 44'\frac{1}{2}$. His Lordſhip was obliged to obſerve the Eclipſe at a Diſtance from the Clock, and to determine the Times by a Pocket Watch, that had been adjuſted by a very good Dial that Day at 12 o'Clock; but aſſures me that the Duration of the annular Appearance was 6′, as near as could be judged by a Watch that did not

 ſhew

ſhew the Seconds. The Moon appeared to touch the larger Spot above-mentioned at $3^h\ 4'$ and covered it in about half a Minute. The Emerſion of the ſame Spot was at $4^h\ 13'$. A leſſer Spot, higher on the Sun's Diſk, was not covered till $11'$ after the greater Spot, but appeared rather ſooner than it.

At *Crosby*, on the Weſt Coaſt of *Scotland*, about 4 Miles North from *Aire*, Mr *Fullarton* obſerved the Eclipſe to begin at 2 o'Clock. A diſtinct *Annulus* was formed about $20'$ after 3, which continued exactly $7'$, meaſured by a Pendulum vibrating Seconds. It appeared rather broader on the lower Verge of the Sun; but the Difference muſt have been very ſmall, for it was but barely diſcernible in a Species of the Eclipſe 6 Inches over, caſt on a Piece of Paper behind the Eye-piece of a Teleſcope 6 Feet long. He adds, that the Day-light was not greatly obſcured, appearing only ſo much dimmer than uſual, as that of the Sun is, when ſeen through a very gentle Miſt in a fine Morning in *April* or *May*. Sir *Thomas Wallace* found that the annular Appearance continued at his Houſe near *Lockryan* in *Galloway* $5'$.

From the Obſervation at *Crosby*, the Centre of the annular Penumbra ſeems to have entered *Scotland* not far from *Irwine*. It proceeded afterwards towards the Eaſt, with a conſiderable Inclination Northwards; and probably left *Scotland* not far from *Montroſe* on the Eaſt Coaſt: For the Reverend Mr *Auchterlony* found, that the annular Appearance continued there $7'$, as near as he could judge by an ordinary Watch. The *Annulus* alſo appeared to him of an uniform Breadth, through a common Teleſcope. This Obſervation, though not ſo exact as that at *Crosby*, is however confirmed by that at *St Andrew*'s, to be mentioned afterwards. Theſe two Obſervations at *Crosby* and *Montroſe*, were made nearer the Path of the Centre, than any others that have been communicated to me.

As for the Southern Limit of this Appearance, the Eclipſe was not annular at *Newcaſtle*, and there wanted about 40 Degrees of the Limb of the Sun to appear in order to form an *Annulus*, according to the Obſervation of Mr *Iſaac Thömpſon*. The whole Duration of the Eclipſe was $50''$ leſs by his than by our Obſervation; and the bigger Spot was hid $1^h\ 9'\ 35''$ by his Obſervation, the Digits eclipſed at it's Immerſion 7, 7; at it's Emerſion 4, 1. Nor was the Eclipſe annular at *Morpeth*, whence Mr *John Willſon* writes, that the Body of the Moon appeared almoſt entirely on that of the Sun; and that to the naked Eye, the Diſk of the Sun ſeemed to be almoſt round.

But of all the Obſervations that have been communicated to me, that of Mr *Long* at *Longframlington* *, determines the Southern Limit with the greateſt Exactneſs. The *Annulus*, he ſays was very ſmall there upon the upper Part, and the Duration 40 or 41 half Seconds, meaſured by a Pendulum 9, 81 Inches long; from which we may conclude,

* *Longframlington* is 7 computed Miles on this Side of *Morpeth*.

conclude, that the Limit was very near this Place. I have received no Accounts concerning this Appearance from any Places on the Weſt Coaſt of *England*. At *Alnwick* in *Northumberland* the Eclipſe was annular, but I have not heard that the Time of it's Continuance was meaſured.

At *Berwick*, the annular Appearance continued betwixt 4 and 5′. The End of the Eclipſe at *Dunbar*, by Mr *Mark*'s Obſervation, was at $4^h\ 48'\ 16''$, but there was ſome Miſtake committed in reckoning the Vibrations of the Pendulum in meaſuring the Continuance of the *Annulus*.

At *St Andrew*'s, this Appearance was obſerved to continue preciſely 6′, by a Pendulum Clock, by Mr *Charles Gregory* and Mr *David Young*, Profeſſors in the Univerſity. By a Figure of the *Annulus* taken from it's Image, projected through a Teleſcope upon a Paper Screen, the Breadth towards the South-eaſt Part of the Sun's Diſk is rather more than double of it's Breadth towards the oppoſite Part.

I have already mentioned the Obſervation at *Montroſe*. At *Aberdeen*, the *Annulus* was obſerved by Mr *John Stewart*, Math. Prof. for 3′ 2″. It was almoſt central, when the Clouds deprived him of any further View of it; he thinks it probable, that it continued there about 6′. Several Gentlemen, who live on the Coaſt Northwards from *Aberdeen*, were deſired to obſerve the Continuance of the *Annulus*; but I do not find that any of them ſaw this Phænomenon from the Beginning to it's End.

At *Elgin*, the Eclipſe was obſerved annular at $3^h\ 29'$ the larger Part of the Ring being uppermoſt, by the Reverend Mr *Irwin*, who had a View of it for about 30″; but by reaſon of intervening Clouds could not determine the Beginning or End of this Appearance. At *Caſtle Gordon*, Mr *Gregory* had one View of the Eclipſe while it was annular, but could make no further Obſervation for the ſame Reaſon.

At *Inverneſs*, the Eclipſe was annular for ſome Minutes, as I am informed by ſeveral Gentlemen; but they did not meaſure the preciſe Time how long it continued. By the Accounts I have had from *Fort Auguſtus* and *Fort William*, it is doubtful whether the Eclipſe was annular in thoſe Places or not. *Fort Auguſtus* is at the Weſt End of *Lochneſs*, and probably was not far from the Northern Limit of this *Phænomenon*. I have as yet received no Accounts of this Appearance from any Place further Northwards, or from any Place in the Weſt, but thoſe I have mentioned. Some Gentlemen in *Argyleſhire*, who obſerved this Eclipſe, were deprived of a View of the *Annulus* by the Clouds.

Mr *Walker*, an ingenious Gentleman at *Frazerburgh* on the North Coaſt, found that from the Time of the Ring's beginning to appear upon the lower and Weſtern Part of the Sun's Diſk, till it began to break on the Eaſt and upper Part, there were 300 Vibrations of a Pendulum,

 or

or $5'$. The Ring feemed fomewhat narrower even at the Middle of the Eclipfe on the lower Part.

This is the Sum of what I have been able to learn concerning the Obfervations of this Eclipfe, that were made in this Country, and in the neighbouring Parts of *England*. I have made fome Computations relating to the Extent of the annular *Penumbra*, and the Direction and Velocity of it's Motion; but fince I have not a fufficient Number of exact Obfervations, by which I might examine them, it would be of little Ufe to defcribe them. Had the Weather been more favourable in the North, and my Requeft of having the Duration of the annular Appearance meafured, been made more public before the Eclipfe, after Dr *Halley*'s Example in 1715; I doubt not but I fhould have been able to have given a more exact Account of the Progrefs of the Centre of this Phænomenon, and of it's Limits; but I had been difcouraged from publifhing any Thing concerning it, by our bad Fortune in feveral late Eclipfes, of which the Clouds had not allowed us the leaft View.

I am informed, that there was very little Notice taken of this Eclipfe by the Populace in the Country; and I cannot but add, that feveral Gentleman of very good Credit, who are not in the leaft fhort-fighted, affure me, that about the middle of the annular Appearance they were not able to difcern the Moon upon the Sun, when they looked without a fmoaked Glafs, or fomething equivalent.

I have taken Notice of this, becaufe it may contribute to account for what at firft Sight appears furprizing, that there are fo few annular Eclipfes in the Lifts collected by Authors. *Kepler*, in his *Aftron. Optic.* does not feem to acknowledge, that any Eclipfe, truly annular, had ever been obferved. There are none mentioned by *Ricciolus*, from the Year 334 till 1567, though there are 13 or 14 total Eclipfes recorded within that Period; yet it is allowed, that the Extent and Duration of the annular Appearance may be confiderably greater in the former, than of the Darknefs in the latter. It may have contributed to this, that annular Eclipfes muft have been rather incident in the Winter Seafon in the Northern Hemifphere, and that Eclipfes have been more readily total in the Summer, when their Chance of being vifible was greater, and the Seafon more favourable for obferving them. But perhaps the chief Reafon why few annular Eclipfes appear upon Record, is, that they have not been diftinguifhed in moft Cafes from ordinary partial ones. The Darknefs diftinguifhed total Eclipfes, or fuch as were very nearly total; and it is thefe chiefly, Hiftorians mention. There are two central Eclipfes of the Sun ftill famous amongft the Populace in this Country: That of *March* 29, 1652 was total here, and that Day is known amongft them by the Appellation of *Mirk Monday*. The Memory of the Eclipfe of *Feb.* 25, 1598, is alfo preferved amongft them, and that Day they term, in their way, *Black Saturday*. There is a Tradition, that fome Perfons

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in the North loſt their Way in the Time of this Eclipſe, and periſhed in the Snow.

There was a remarkable total Eclipſe of the Sun in this Country, *June* 17, 1433, the Memory of which is now loſt among the Populace; but it appears from a Paſſage in a Manuſcript in our Library, that it was formerly called by them the *Black Hour*, after their uſual Manner. It is deſcribed thus: 'This year there was a wonderful 'Eclipſe of the Sun, on *June* 17, about 3 in the Afternoon; and 'for about half an Hour, a Darkneſs like Night overſpread the Face 'of the Earth; ſo that nothing was viſible to human Eyes; whence 'it has commonly been called the *Black Hour*.' This Eclipſe is not in *Ricciolus*'s Catalogue, but is mentioned by him in another Place, *Schol. Cap.* 2. *L.* 5. By a Computation of this Eclipſe, the Sun was within two Degrees of his *Apogeum*, and the Moon within 13 Degrees of her *Perigeum*; ſo that this muſt have been a remarkable Eclipſe. The Progreſs of the Shadow was towards the South-eaſt; and *Sethus Calviſius* cites the *Turkiſh* Annals for it's being total in ſome Part of their Dominions.

P. S. We looked for the Occultation of *Aldebaran* by the Moon on *Feb.* 25, in the Evening; but the Star paſſed by the upper Horn, without being hid, at a Diſtance from it, that was by Eſtimation nearly equal to the Diſtance betwixt the neareſt Part of the Spots *Eudoxus* and *Ariſtotle*.

4. We had a very fine bright Day for obſerving the Eclipſe; and never was any Thing of that kind, I believe, obſerved with more Exactneſs. In ſeveral Places for 10 Miles round this City, as well as in it, were ſome ſkilful Perſons ſtationed for that Purpoſe: I myſelf happened to be in the Caſtle here, which is an Eminence at leaſt of 500 or 600 Feet in Height, beſides a great Aſcent from the Level of the Sea to the Foot of the Rock upon which it is ſituated.

— *At* Edinburgh, *by the Hon. Sir* John Clerk, *Bart. one of the Barons of his Majeſty's Exchequer there, and F. R. S.* Ibid. p. 195.

Mr *Mac Laurin* had placed himſelf at a Window in our College; others were ſent where the Eclipſe we ſuppoſed, would be perfectly central, about 12 or 14 Miles farther North.

A Gun from the Caſtle was fired at 22″ after Twelve, mean Time, (or 12′ 22″ before Twelve, apparent Time) upon which, by Agreement, the Clocks and Watches of the Obſervers were adjuſted. A ſecond Cannon was diſcharged preciſely when the Eclipſe began, which was at 5′ 36″ after Two. A third was diſcharged when the annular Appearance began, which was at 25′ 55″ after Three; it's Continuation was 5′ 48″. A fourth Cannon was fired at the End of the Eclipſe, which was at 44′ 50″ after Four; all reckoned by apparent Time. We had half a ſcore good reflecting Teleſcopes to make theſe Obſervations, and our Calculations perfectly agreed, ſo that you may depend upon them as moſt exact.

This was not done by us as a Matter of mere Curioſity, but to aſſiſt in aſcertaining the Motions of the Moon, on Sir *I. Newton*'s

Theory

Theory upon which a good deal of the Doctrine of the Longitude will depend. Sir *Isaac*'s Calculation, as to the Beginning of this Eclipse, was pretty right; but not so well as to it's central Appearance. Two Spots in the Sun made a very distinct Appearance to us, as they entered under the Moon's Body; one was a little above the central or horizontal Line of the Sun, shaped as in the Figure; the other was near the Edge, on the East Quarter. The first, by Comparison with the Sun's Diameter, was larger than the Disk of our Earth; it was dark in the Middle, and certainly emitted no Fire or Light. The Edge of the Moon appeared a little ragged or rough, but not mountainous, because of the Sun's Light. There was no considerable Darkness, but the Ground was covered with a kind of a dark greenish Colour. Two Stars appeared, the Planet *Venus*, and another farther Eastward. This Account is what you may depend on.

Fig. 65.

—*At* Trinity-College, Cambridge, *and at* Kettering; *communicated in a Letter from Mr* Charles Mason, Ibid. p. 197.

		h	′	″	
5. The Beginning by the Clock	— — —at	2	36	40	
The End	— — — — — — —at	5	14	12	Exact.

Digits.	The Eclipse observed. Increasing. h	′	″	Decreasing. h	′	″
0½	2	39	30	5	11	50
1	2	43	00	5	9	00
1½	2	46	40			
2	2	50	25			
2½	2	54	15	4	59	30
3	2	58	5			
3½	3	1	55			
4	3	5	50			
4½	3	9	50			
5	3	14	00	4	42	55
5½	3	18	10	3	39	10

Digits.	The Eclipse observed. Increasing. h	′	″	Decreasing. h	′	″
6	3	22	20	4	35	20
6½	3	26	30			
7	3	30	40	4	27	40
7½	3	34	50	4	23	55
8	3	39	30	4	20	10
8½	.	.	.	4	16	30
9	Clouds.			4	13	00
9½	.	.	.	4	10	10
10	.	.	.	4	7	50

		h	′	″
The lesser Spot immerged	— — — — — —at	2	58	50
The greater Spot began to immerge	— — — —at	3	33	05
The Middle	— — — — —at	3	33	20
The End at	— — — — — —	3	33	37

Times

Pla. X. Vol. VIII. part 1. pa. 150.

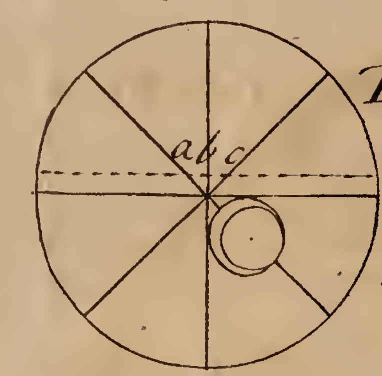


Fig. 60.

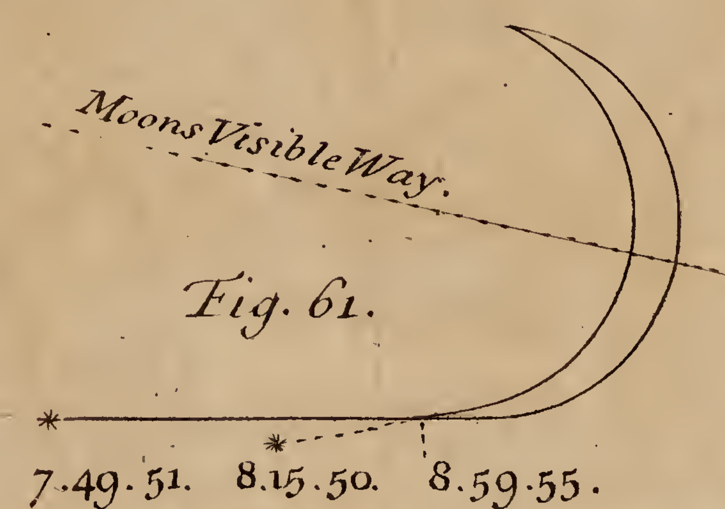


Fig. 61.

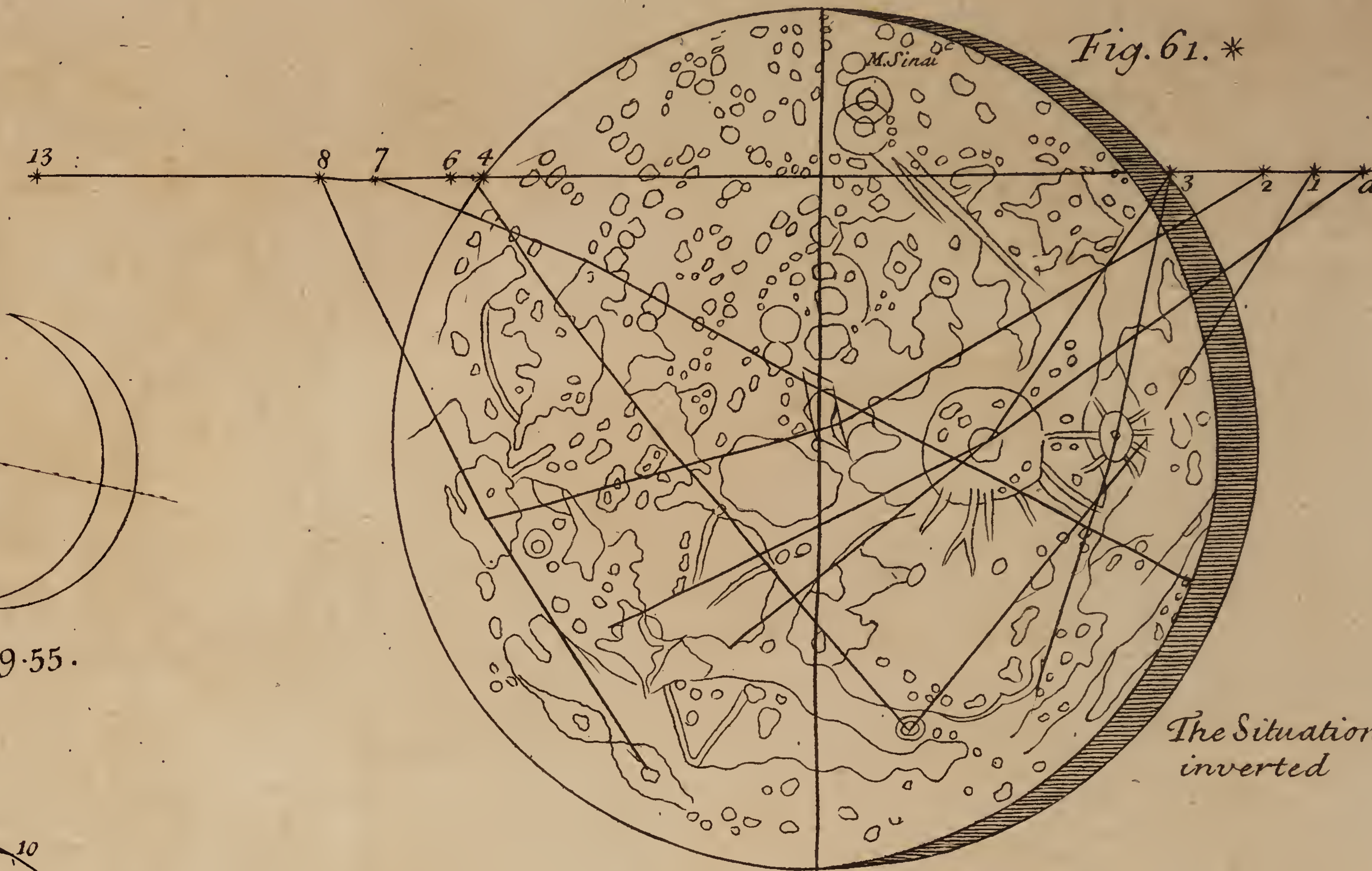


Fig. 61. *

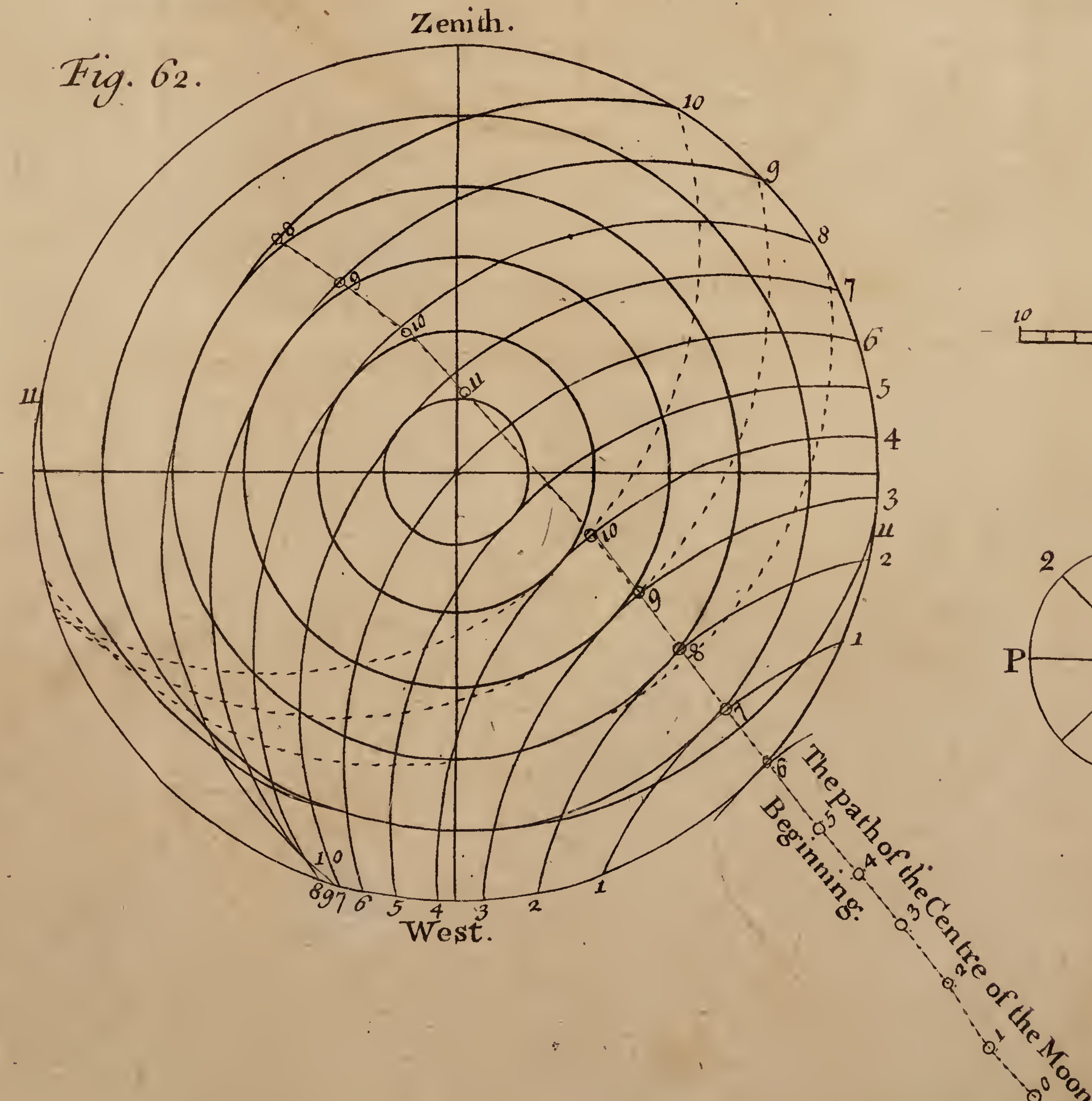
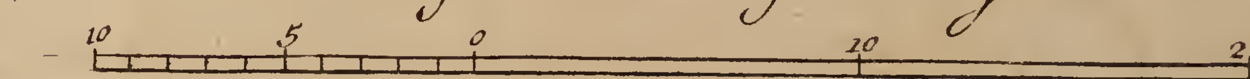


Fig. 62.

Scale of Minutes of a Degree.



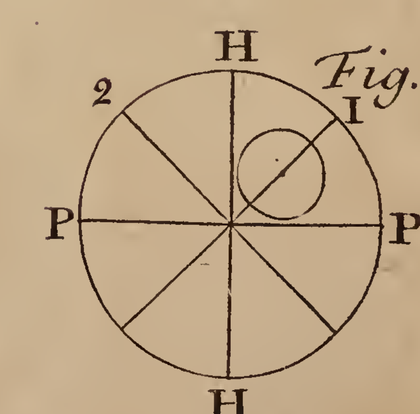


Fig. 63.

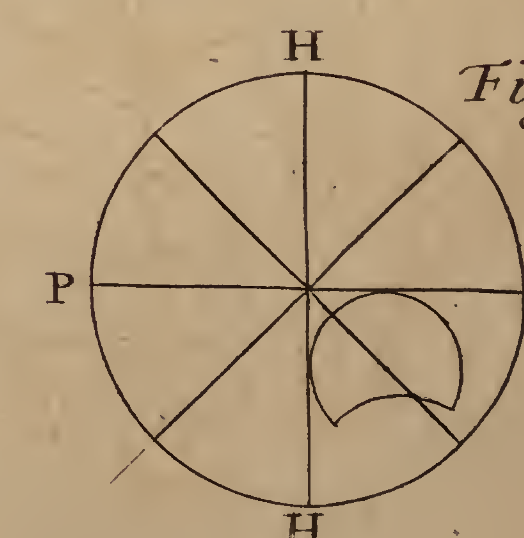


Fig. 64.

Fig. 65.

Times obferved at *Kettering*, as follow:

Beginning — — — — — — — —	2	21
2 Digits — — — — — — — —	2	36
Centre — — — — — — — —	{ 3	07
	4	22
End — — — — — — — — —	4	59
Great Spot immerfed — — — — — —	3	18

N. B, The Obfervatory Clock was $1' 50''$ too flow, which being added all the way will give true Time.

6. The Beginning of this Eclipfe was above feven Hours fooner than by our Calculations. For at $3^h 33' 36''$, part of the Sun's Limb feemed to be obfcured by the Moon, as we looked through a fmoaked Glafs, fitted to a Telefcope of 11 Feet, whereas but a little before, at $33''$, the Sun appeared quite round thro' the fame Telefcope. But the Calculations placed the Beginning of the Eclipfe at $3^h 41'$. — *At* Bologna, *by* ——— Ibid. p. 199.

We then obferved the Digits of the Eclipfe on a white Table, upon which the Rays of the Sun were thrown, by an Optical Tube of 6 Feet; there was a Circle infcribed on the Table, meafured by the Image of the Sun, and divided into Digits and half Digits. The Obfervation was pretty much difturbed by the Wind fhaking the Inftrument. The following feem to have been the moft certain Phafes.

h	′	
3	40	about one Digit was eclipfed.
3	48	two Digits.
3	57	three Digits.
4	6	four Digits.
4	15½	five Digits.
—	—	
4	35	feven Digits.
4	45	feven Digits ½ which feemed to us the greateft Darknefs.
4	55	feven Digits again, the Eclipfe now decreafing.

When the Appearance of the Sun going down began to appear too fluctuating and trembling, and disfigured from a round into an oval Shape, we left off meafuring the Digits, becaufe it was not attended with fufficient Certainty.

Some Spots appeared in the Sun, efpecially 3, the Pofitions of which at Noon that Day, being defcribed from the Obfervations, is exhibited

Fig. 66. exhibited in the Scheme. We have thus determined the Occultations of two of these by the same Tube of 11 Feet.

h	′	″	
4	23	18	The Limb of the Moon touches the *Corona* of the Spot A.
	23	49	It begins to touch the *Nucleus* of the Spot A.
	24	25	It hides the whole *Nucleus.*
	26	14	It touches the Spot B.
	26	31	It covers the whole.

—*On Mount* Aventine *at* Rome, *by the Abbot* Didacus de Ravillas, *F. R. S.* Ibid. p. 200.

7. The Image of the Sun was thrown thro' a Telescope of *Campanus* upon a white Table, with a Circle equal to the Image, divided into 12 Digits. The Phases observed by this Instrument are as follow.

h	′	″ p. m.	
3	43	4	The Limb of the Sun was found to be a very little obscured by the Limb of the Moon.
	51	50	one Digit.
4	0	40	two Digits.
	9	30	three Digits.
	18	20	four Digits.
	27	10	five Digits.
	30	00	six Digits, whilst the Limb of the Moon touches the Centre of the Sun, thick Clouds take away the Sight of both Luminaries, and of the succeeding Phases of the Eclipse.

——*At* Wittemberg *in* Saxony, *by* J. Frederick Weidler. Ibid. p. 201. Fig. 67.

8. Digits of the decreasing Eclipse

Digits	h	′	″ p. m.
8	4	50	31
7 ¾		58	16
7 ½	5	1	36
7 ¼		5	26
7		8	16
6 ½		10	16

Afterwards, as the Sun went down, it was hid in Clouds. The Beginning could not be seen because of Clouds.

—*At* Philadelphia *in* Pensylvania, *by Dr* Kearsly. No. 446. p. 121. July, &c. 1737.

9. The Eclipse *Feb.* 18. could not be well observed here, by Reason of Clouds. I rectified my Clock by one of *Heath*'s large Ring Dials. At 7^h $18'$ there was a small Dent in the Sun's Edge, whence the Beginning 1 or $2'$ sooner: Just before the End, *viz.* 10^h 11 or $12'$, I had a Sight of the Sun again, and there was then a Dent in the Sun's Edge, so that the End must be 10^h 13 or $14'$ in the

the Morning: About the Middle of the Eclipse, there was a large Spot near the Middle of the enlightened Part which was the North Side of the Sun.

VII. 1. This Observation was made by a Refracting Telescope of 12 Feet *Focus*, armed with a Micrometer, and by a reflecting Telescope of 9 Inches focal Length.

Eclipse *of the* Sun, *observed* Aug. 4, 1738. *by Mr* George Graham *and Mr* Short, *FF. R. S. at Mr* Graham's *House in* Fleetstreet, London. No. 453. p. 91. April, *&c.* 1739.

	h	′	″ a. m.
Beginning of the Eclipse at — — — — —	9	59	20
End at — — — — — — — — —	11	59	36

	dig.	min.
Quantity of Obscuration by the Micrometer — —	3	28

	h	′	″
Duration — — — — — — — — —	2	00	16

N. B. The Person who was observing the Transit of the Sun over the Meridian, observed the End to be at the same Instant with the above Observation.

2. This Observation was made with a Tube of 7 Feet, armed with one of Mr *Graham*'s Micrometers.

— *At* Upsal, *by* Andreas Celsius, *F. R. S and R. S.* Suec. *Secr.* Ibid. p. 92.

True Time.

h	′	″			
12	18	52	Beginning of the Eclipse.		
12	35	57	Digits eclipsed	0	5⅖
12	37	47		0	3⅕
12	42	22	End.		
0	23	30	Duration.		

Because of the Clouds that covered the Sun at times, I could not observe the greatest Darkness and other Phases of the Eclipse; but we may deduce from these Observations, that the greatest Obscuration was 0 8′ Digits at 12^{h} 30′ 37″.

3. I could not observe either the Beginning or End of the Eclipse, because of the Clouds; but as they were sometimes broken by the Wind, I had an Opportunity to observe the following Phases.

— *At* Wittemberg, *by* Joh. Frid. Weidler, *F. R. S.* Ibid. p. 92. Fig. 68.

h	′	
11	30	The first Phase of the increasing Eclipse was observed, 1 Digit.
12	19	p. m. Another Phase was seen, 2 Digits 30′.
12	37	The third Phase of the decreasing Eclipse was seen.

There were also seen at the same Time 10 Spots in the Disk of the Sun.

 The

The Disk of the Moon under the Sun shewed the Circumference exactly terminated, without any Inequality, and very black. No Trace of any Atmosphere on the Orb of the Moon could be perceived.

The Calculation taken from the Ludovician Tables erred both as to the Magnitude and Time: For the Magnitude was predicted to be 2 Digits 20′; and the Middle to be $12^h\ 5'$.

—*At* Bologna, *by* Eustachius Manfredi, *F. R. S.* Ibid. p. 94.

4. As the Disk of the Sun abounded with Spots at this Time; on the Morning of the approaching Eclipse, about $21^h\ 30'$ p. m. *Eustachius Zanotti Phil. Doct. and Math. Prof. Publ.* my Collegue, traced out the Position of the chief of them, by the Help of a Micrometer, fitted to a Tube of 8 Feet. These occupied chiefly the southern Part of the Sun, which the Moon was to cover. It was not necessary to describe them all, nor could it be done for the Multitude of Spectators. Those, of which the Places could be determined, are shewn in the Scheme.

Fig. 69. The Beginning of the Eclipse was not perceived before $22^h\ 52'\ 25''$, p. m. tho' I had long observed it with a Tube of 11 Feet, and others with other Tubes. I am of Opinion however, that the Contact of the Luminaries happened at least a Minute sooner, than I perceived it; which seems to be confirmed by the succeeding Phases.

The Digits described by Circles on a Table, after the usual Manner, and the Parts of Digits are determined by Estimation. The Telescope was 6 Feet; the Image was 2 Inches or thereabouts. The Phases of the Emersion are more certain than the Phases of the Immersion for many Reasons.

Phases of the Immersion. True Time. h ′ ″					Phases of the Emersion. True Time. h ′ ″			
23	0	10	dig. 1		0	4	14	dig. $4\frac{4}{5}$ still
	11	20	2			18	5	$4\frac{1}{2}$
	25	56	3			22	43	$4\frac{1}{3}$
	35	14	4	doubtful		31	50	4
	45	14	$4\frac{1}{3}$			39	13	$3\frac{1}{2}$
	47	6	$4\frac{1}{2}$			46	50	3
	51	14	$4\frac{3}{5}$			52	55	$2\frac{1}{2}$
	55	14	$4\frac{2}{3}$			57	31	2
	58	14	$4\frac{3}{4}$		1	3	26	$1\frac{1}{2}$
0	1	46	$4\frac{4}{5}$			7	52	1
					1	13	4	$\frac{1}{2}$

	h	′	″
End of the Eclipse with a Tube of 11 Feet	1	18	1
8 Feet	1	18	2

In

Fig. 66.

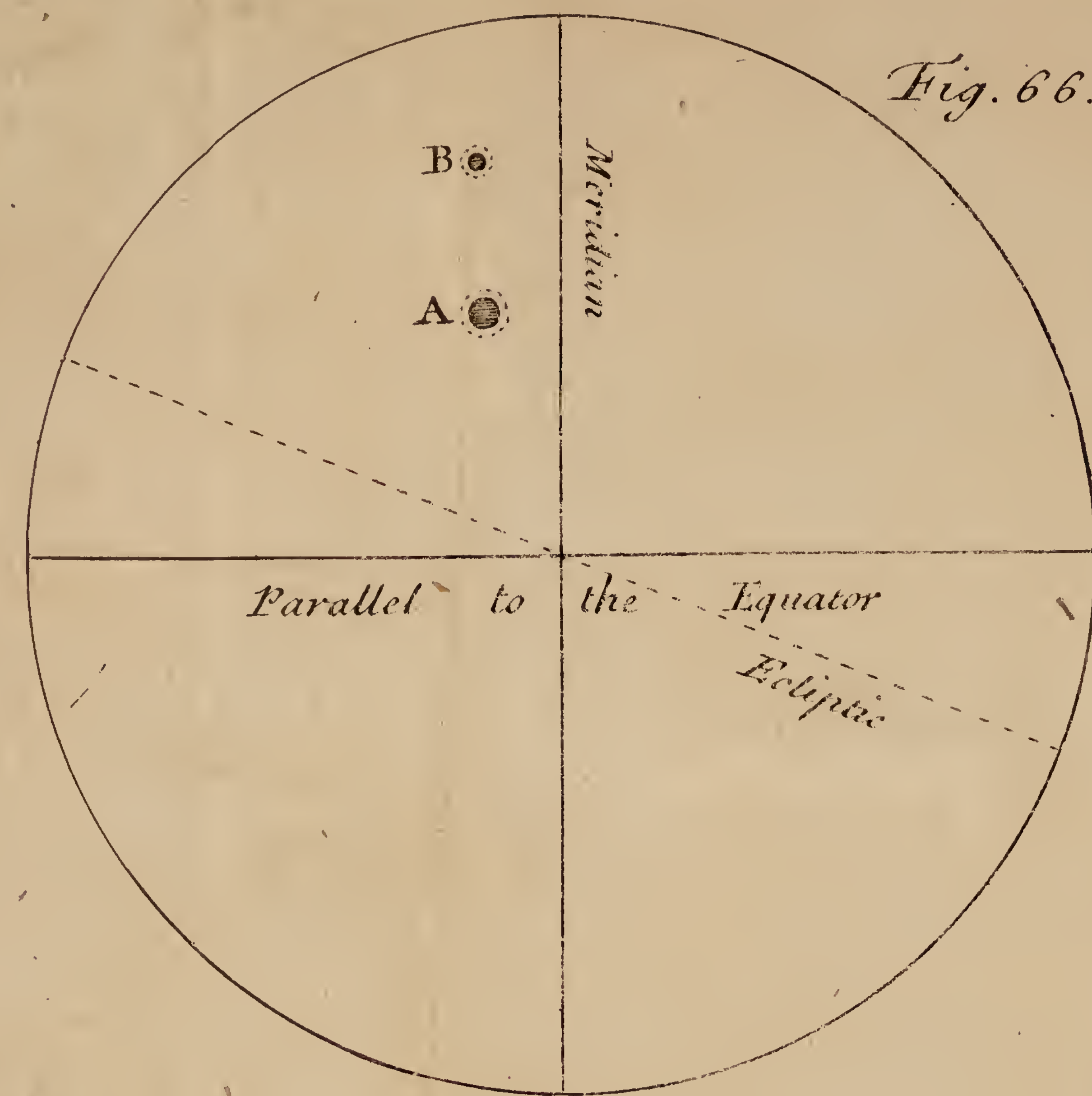


Fig. 67.

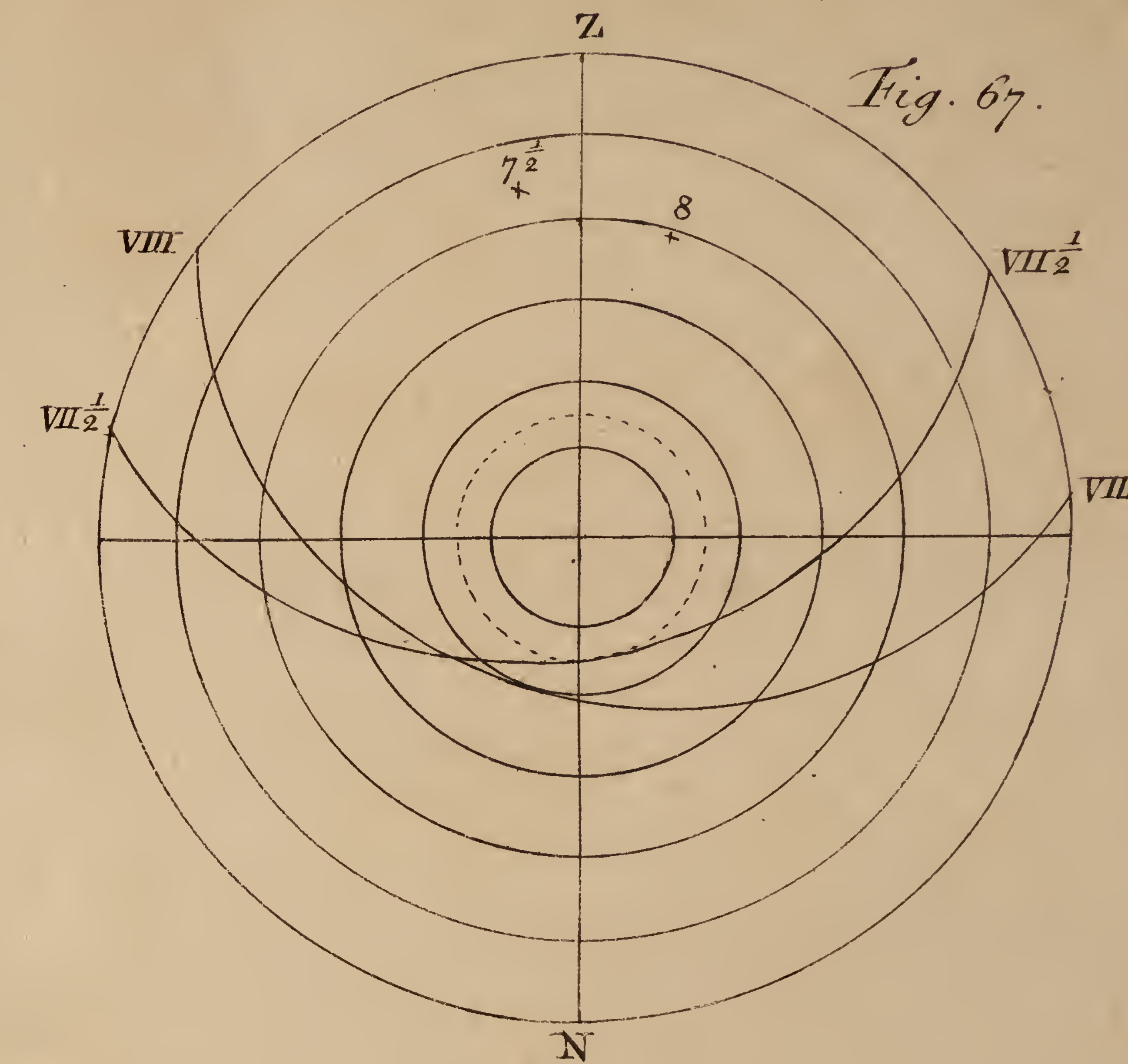


Fig. 68.

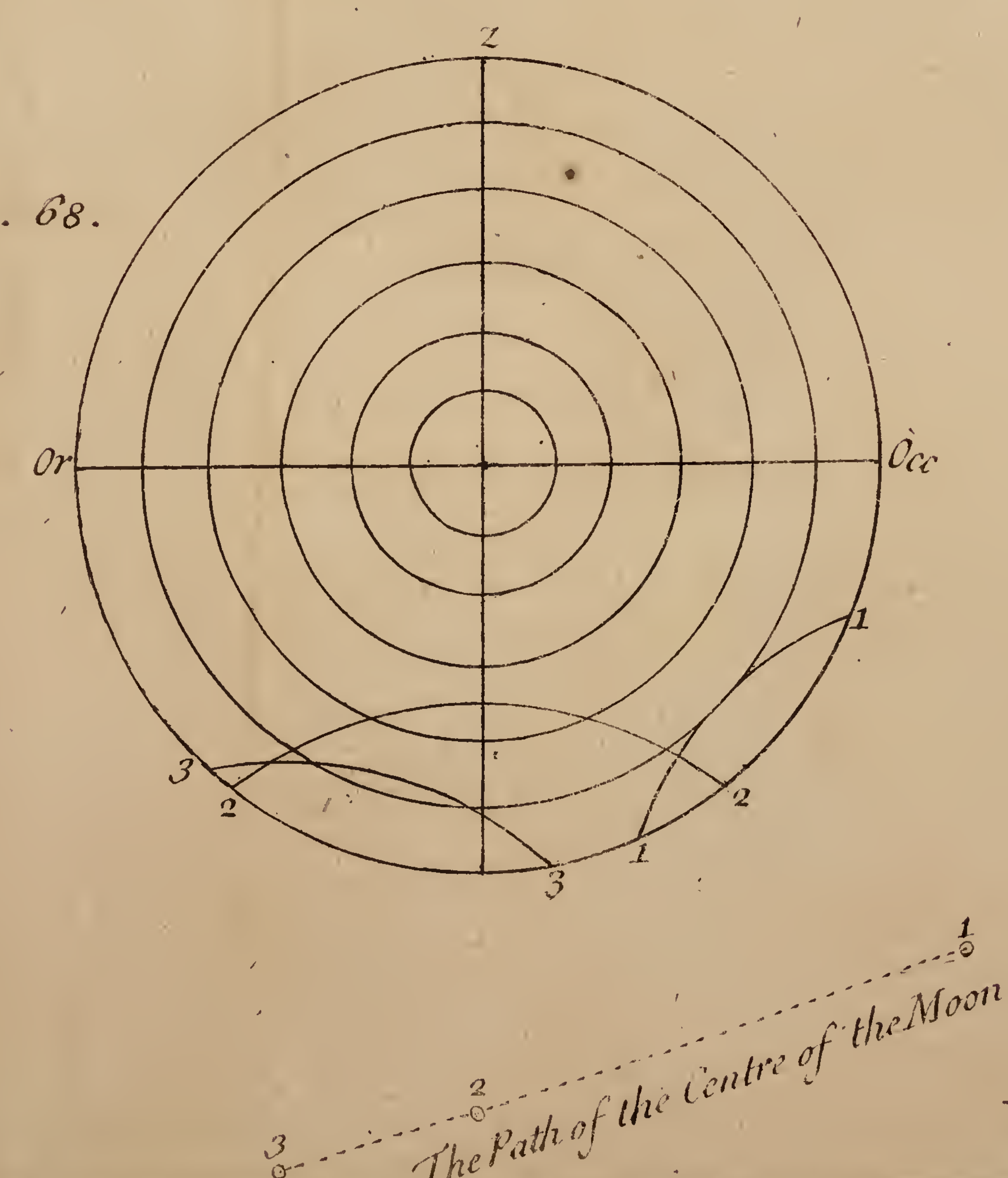
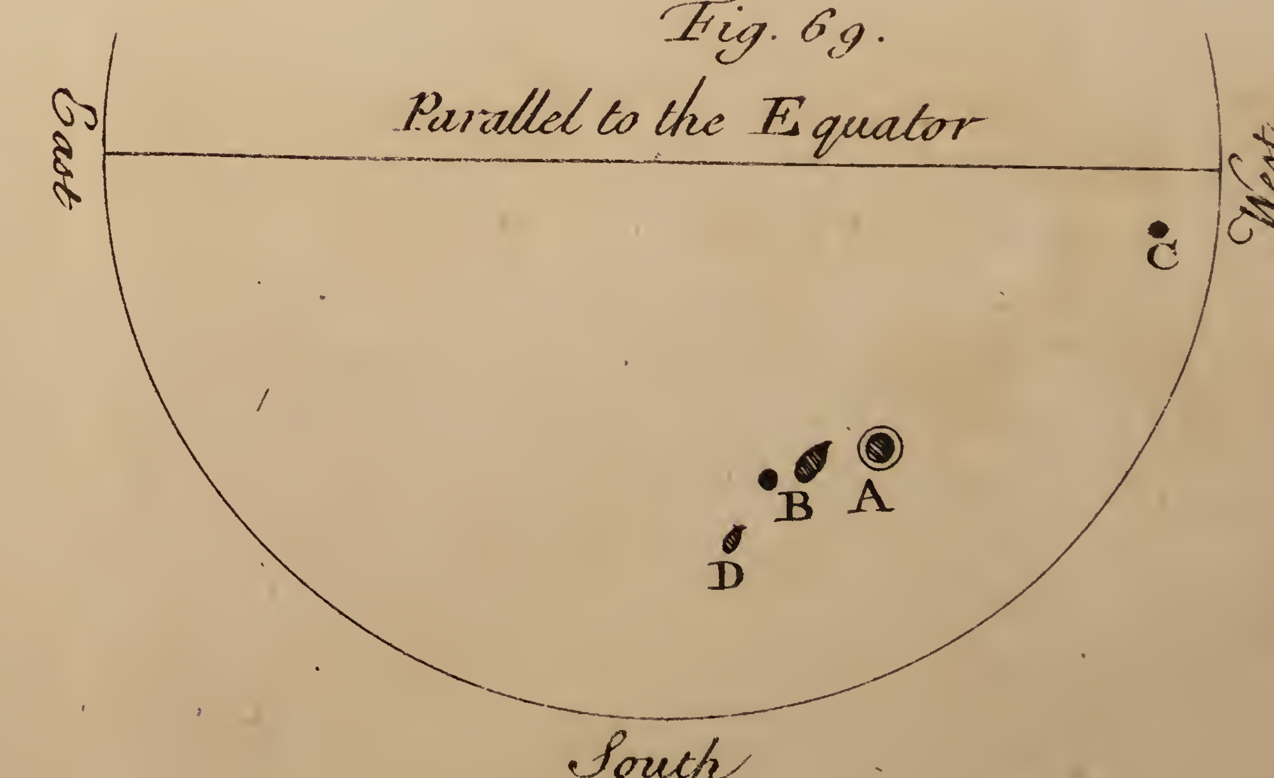


Fig. 69.



In the mean Time, the Spots of the Sun were covered and uncovered after the following Manner.

True Time.			
h	′	″	
23	3	50	Spot C covered by the Moon, with a Tube of 8 Feet.
	21	3	Spot A begins to be hid, with a Tube of 11 Feet.
	21	49	The Centre of the Spot A is hid.
	22	41	The whole Spot immerges.
	23	54	The firſt of the 2 Spots at B begins to immerge.
	25	10	The Centre of the ſame Spot is hid.
	25	45	The whole Spot is hid.
	26	24	The latter of the 2 Spots at B touches the Limb of the Moon with it's Centre. Hitherto I obſerved with the ſame Teleſcope of 11 Feet.
	27	2	The Spot D begins to be hid, with the Tube of 8 Feet.
	31	2	The whole Spot is hid with the ſame Tube.
0	31	45	The Spot A begins to appear on the Image of the Sun thrown on the Table.
	32	30	The ſame Spot had entirely emerged, with it's Ring with the Tube of 11 Feet.
	33	25	Emerſion of the Centre of the firſt of the two at B.
	34	59	Total Emerſion of the ſame Spot.
	35	51	Total Emerſion of the latter; all theſe with the ſame Tube of 11 Feet.

The Obſervations both of Spots and Digits were made by ſeveral other learned Men beſides *Zanotti*; and all obſerved the Time by the ſame Clock, which was afterwards corrected by Obſervations of the Meridian.

During the Eclipſe, I obſerved the Tranſit of the Moon over the Sun by the Plane of a mural Semicircle ſuſpended at the Meridian.

To determine the Tranſit of the Moon, I noted the Time, when a very ſmall Segment of the Diſk of the Moon, viſible upon the Sun, under the horizontal Thread of the Teleſcope, appeared to be biſected by the vertical Thread; for then the very Centre of the Moon muſt have been on the vertical one. But the Centre of the Moon paſſed over the Centre of the Sun at $23^h\ 59'\ 26''$. p. m. The Meridian Altitude of the Northern Limb of the Moon was $59^\circ\ 36'\ 15''$; of the Northern Limb of the Sun, $59^\circ\ 53'\ 0''$.

Eclipse *of the* Sun *observed at* Wittemberg *in* Saxony, July 24, 1739. *by* Joh. Frid. Weidler. No. 454. p. 226 July, &c. 1739. Fig. 70.

VIII. Increasing Phases.

h	′	″	p. m.
4	15	30	Beginning.
	22	00	dig. 1
	29	30	2
	35	30	3
	40	00	4
	47	30	5
	55	40	6
5	2	00	7
	9	00	8
	24	40	9

Decreasing Phases.

h	′	″	p. m.
5	35	30	dig. 8
	43	40	7
	50	30	6
	56	00	5
6	2	45	4
	8	40	3
	14	00	2
	20	45	1
	27	20	End.

Observation of the Immersion and Emersion of the Spots, which were conspicuous on the Disk of the Sun at the Time of the Eclipse. Fig. 71.

Immersions.

h	′	″	
4	34	35	Appulse of the Moon to the Spot *a*.
	34	45	The whole Spot *a* is covered.
5	1	30	Appulse of the Moon to the Spot *d*.
	5	20	——————— to the Spot *e*.
	7	15	——————— to the Spot *b*.
	10	00	Total Immersion of *b*.
	16	30	Appulse of the Moon to the Spot *c*.
	18	00	Total Immersion of the Spot *c*.

Emersions.

h	′	″	
5	30	50	The Spot *b* begins to emerge.
	32	30	The Middle of the Emersion of *b*.
	34	00	Total Emersion of *b*.
	39	00	The Spot *c* begins to emerge.
	39	50	Middle of the Emersion of *c*.
	40	40	Total Emersion of *c*.
	41	00	The Spot *a* begins to emerge.
	41	40	Total Emersion of *a*.
6	4	30	Emersion of *d*.
	6	15	Emersion of *e*.

Fig. 70. Shews the Disk of the Sun in the Situation in which it appears thro' the Helioscope.

Fig. 71. Represents the Spots of the Sun in that Situation which they had about the Beginning of the Eclipse; of which the Immersion and Emersion were observed during the Eclipse.

The Moon came upon the Sun at about 102° from the Zenith; and went off at about 53° from the same Zenith.

At the Time of the greatest Darkness the Orb of the Moon did not appear

appear quite black thro' the Telescope, but tinged with red; but the Spots of the Moon were not distinguishable.

The Edge of the Moon on the left Side toward the South, about the Time of the greatest Darkness, shewed the Tops of it's Mountains, which were also perceivable in the Image painted by the Telescope. The rest of the Edge appeared even.

During the whole Eclipse, the Circumference of the Moon appeared naked, without any Mist or Cloud, which sometimes hang over it in other Eclipses. But about the End, when one Digit about the Disk of the Sun was still hid, there was a vehement Motion of the Solar Light on the rough Edge of the Moon.

In the last Place, I must not omit, that a Friend of mine very skilful in these Affairs, who viewed the Sun thro' a Telescope of 9 Feet, about $4^h\ 31'$ observed a Light in the dark Disk of the Moon resembling Lightning; and that the same Observer about $5^h\ 50'$, affirmed to all the Company, that he saw again 3 Times such Coruscations breaking out on a sudden.

IX. The Observation was made with a reflecting Telescope of 16 Inches Focus, that magnified about 40 Times.

Eclipse *of the* Sun, Dec. 19. 1739. *in the Morning, observed by Mr* Short *in* Surrey-street.

The Beginning could not be seen for Clouds about the Horizon.

About $35'$ after 8 o'Clock, there was an Opening, when the Sun seemed to be about 2 or 3 Digits eclipsed.

End was exactly observed at $9^h\ 1'\ 45''$, Time apparent.

No. 459. p. 633. Jan. &c. 1741.

X. A Projection of the Arches and Circles, conceived upon the illuminated Hemisphere of the Earth, upon a Plane, may serve very well to shew any Eclipse of the Sun; and if the Places situated on the Surface of the Earth, as Cities, Shoars, Islands, &c. are inserted in the Projection, and if a Circle is added, to express the Position and Magnitude of the Lunar *Penumbra*, and some smaller Circles concentrical with it, we have then in one View those Places, where the Sun is covered by the Moon, and where any Part of it is withdrawn from our Sight.

An Instrument to represent Eclipses *of the* Sun, *by* J. And. Segner, *Med. Phys. and Math. Prof.* Goetting. *and F. R. S.* No. 461. p. 781. Aug. &c. 1741.

But such an Image is momentary, and as it shews with great Accuracy what happens at any precise Point of Time; as for Instance, when the Centre of the Lunar *Penumbra* first enters the Disk of the Earth, it cannot exhibit the other *Phænomena*, which depend, partly on the Rotation of the Earth, partly on the Motion of the Moon. Thus if we would exhibit in this Manner all the Appearances of an Eclipse, as they succeed each other, we must delineate a great Number of Projections; which would be an Affair of infinite Labour, and would hardly be recompensed by the Pleasure expected.

Whilst the Earth turns round, the Circles of Latitude indeed, and consequently the Projection of them, remain the same; but the Meridians, or Circles of Longitude, are continually changed, and consequently

ſequently the Projection of them, and the Situation of the Places of the Earth, ſo far as depends upon them.

But the artificial Globe of the Earth, ſhews the Hemiſphere illuminated by the Sun at any Point of Time, with very little Trouble. For the Pole being elevated above the Horizon, or depreſſed below it, ſo that the Elevation or Depreſſion may be equal to the Declination of the Sun at that given Time; or, which comes to the ſame End, the Sun's Place being put in the Ecliptic of the Globe in it's Zenith, the artificial Horizon becomes the Boundary of the Light and Shade; for it diſtinguiſhes the illuminated Hemiſphere of the Earth from the dark one, and nothing remains to exhibit plainly the illuminated Hemiſphere, but to turn the Globe round upon it's *Axis*, till it obtains the Situation which the Hour of the Day requires.

Thus what is very difficult in Projections, is with great Eaſe performed by the Globe, and alſo more conformably to Nature. When I conſidered this, I found we ſtill wanted, in order to repreſent all the *Phænomena* of any Eclipſe of the Sun, to project the Lunar *Penumbra* upon the Globe, and to make an Inſtrument, to repreſent the Situation of it at any Time, and to refer it to thoſe Places of the Earth which are marked upon the Globe. By which Facility of doing the Thing, I was induced to think of ſuch an Inſtrument; and accordingly I have attempted to execute it after the Manner repreſented in *Fig.* 72.

Fig. 72.

It is a common terreſtrial Globe, furniſhed with it's Horizon, Meridian, and Hour Circle. To the Horizon are faſtened two wooden Arms, A B, *a b*, in Length a little exceeding the Semidiameter of the Globe; one End of each of theſe Arms, is made to embrace the Horizon, and may be faſtened to any Part of it by Means of Skrews, one of which is ſhewn at D.

On the oppoſite Extremities B *b*, are placed wooden Columns, perpendicular to the Horizon B E, *b e*, of the Height of the Semidiameter of the Globe, and of the Breadth of the Brazen Meridian, ſo that a right Line being drawn thro' the Tops of the Columns cannot touch the Meridian.

On the Top of each Column is a little Ball of Braſs; each of theſe Balls is perforated by an Iron *Axis*, appearing on both Sides, and firmly joined to the Ball. The lower Parts of the *Axes* were fixed into the Columns, ſo that the Balls are held faſt in a Situation parallel to the Horizon of the Globe.

The upper Parts of the *Axes* are round and poliſhed, as well as the upper Surfaces of the Balls; and receive round Plates of Braſs E F G, *e f g*, which reſt upon the Balls in ſuch a Manner, that being turned round the *Axes* they always remain parallel to the Plane of the Horizon. The Plates are about 3 Inches in Diameter, and each of them has a Notch in the Circumference, to receive a Thread. The Plate *e f g*, is ſomething leſs than the Plate E F G; and this Diffe-

ference

rence in Magnitude is no Injury to the Instrument. Besides it has nothing Particular in it; and therefore it is only fastened with a Ball to keep it from falling off the *Axis.*

But the other Plate E F G has a Circle inscribed upon it, divided into Degrees, and an Index H is added, to shew the Number of those Parts. This is so situated, as to turn round the *Axis* without moving the Plate, or being affected itself by any Motion of the Plate. In order to this, a little immoveable Ball is placed between the Plate and the Index, for the Index to turn round upon it any Way.

Then there are three Rays of Brass, *i k*, *i l*, *i m*, connected in *i*, containing equal Angles *k i l*, *l i m*, *m i k*; and the Plane *i* is perforated with a very small Hole. The Rays are elastick, and as thin as could be made to be firm, and nearly of the Length of ¼ Part of the Globe. The Rays have also little Perforations at *l* and *m*, thro' which a Thread being drawn is brought round the Plates by *m* F G *g f e l*, the Ends being fastened together between *l* and *m*, wherefore the Skeleton of the *Penumbra* is also rendered immoveable at the Part of the Thread *e l m* E, it's third Ray lying freely on the Part of the Thread *g* G; hence the Skeleton is turned either away in a right Line, upon the Turning of the Plate E F G, or *e f g*.

By this Construction might be discovered how many Parts of the Division of the Plate E F G would answer to the Diameter of the Globe, after this Manner. The Arms A B, *a b*, are so placed, that upon the Skeleton's being moved, it's Centre *i* would run thro' the Diameter of the Globe; and to effect this, the Horizon of the Globe is placed in a Situation parallel to the Horizon of the Earth, and a Pendulum *i n* is let fall from that Centre, to shew the Points of the Horizon, over which the Centre would hang. Therefore moving the Centre forward, according to the whole Length of the Diameter of the Globe, we might note the Number of Parts of the Plate E F G, which have passed in the mean Time thro' the Index H; which being carefully observed, must be retained in Memory, since the Use of it, as well as of every Thing that has hitherto been described, will occur in the Representation of all Eclipses. These that follow must be changed according to each particular Eclipse.

The Principal of these is the Disk of the *Penumbræ*, which I have endeavoured to effect after the following Manner. Having found, by the Tables for the Eclipse which I would represent, the Semidiameter of the Lunar *Penumbra* on the Disk of the Earth, as also the horizontal Parallax of the Moon, I argued thus; as the horizontal Parallax of the Moon is to the *Radius* of the penumbrous Disk, so is the Semidiameter of the terrestrial Globe, that I made use of, to the Quadrant, which expressed the *Radius* of the *Penumbra* required by the Magnitude of the Globe.

As the Size of the Instrument seemed not to admit of a Division into 12 Parts, I divided that *Radius* into 6, and described concentri-

cal Circles on a thicker Paper, which I cut into *Armillæ* according to them. I pasted the biggest of these to the Skeleton *k l m*, so that the Centre might agree with the Centre of the Skeleton *i*; then I rejected the second, and pasted the third to the Skeleton in the like Manner, and rejecting the fourth, I pasted on the fifth, rejecting also the inner Circle; so that the Figure might arise, as it is described between *k l m*. The Use of it is to shew, that all the Places marked upon the Globe, which lie under the outer Edge of the greatest Circle, see the Beginning or End of the Eclipse, that those which are situated under the inner Edge of the same Circle, see 2 Digits eclipsed; that those which lie under the outer Edge of the second Circle have an Eclipse of 4 Digits, and so on; but that those which lie under the Centre see the Eclipse total; for I have thought it sufficient to mark the Shadow, because of it's Smallness, thro' the very Centre.

To set every Thing in order for any Moment of a given Eclipse, we must proceed in the following Manner. Having found by Calculation the Points of the Bound of Light and Shadow, by which the Centre of the Moon first enters the Disk of the Earth, and again departs from it, they are to be marked on the Horizon of the Globe, and the Arms A B, *a b*, are to be placed so that the Plate E F G being turned round, the Centre *i* of the Disk of the *Penumbra k l m* may pass over them; and whether this is done or not, will be shewn by the Pendulum *i n*. Then I find the Time when the Centre of the *Penumbra* is in any remarkable Place, as when it first enters the Disk of the Earth, and place the Globe, by means of the Meridian and Equator, without the Help of the Hour Circle, in such a Manner, that the Part above the Horizon may shew the Hemisphere of the Earth at that Time illuminated by the Sun. I then turn the Plate E F G till the Centre of the *Penumbra i*, is perpendicularly over that remarkable Place, as the Bound of Light and Shadow, for Example; which I call the primary Situation of it, and this being obtained, I move the Index H of the Plate to the Beginning of the Division. Thus every Thing is rectified for this Time, and it's *Phænomena* may be collected.

Now the horary Motion of the Moon from the Sun, being taken from the Tables, I infer, that as the horizontal Parallax of the Moon, is to this horary Motion of the Moon; so is the Number of Parts of the Plate E F G, which answers to the Semidiameter of the Globe, found above, to the Quadrant, which shews how many Parts, upon turning the Plate round, are to be drawn thro' the Place of the Index, that the Situation of the Disk of the *Penumbra* may be had, an Hour before or after the Time, which answers to the primary Situation. The Disk therefore being brought to this Place, and the Globe being turned round the Axis, the *Phænomena* of this Time may be had in like Manner.

Now

Now the Situations of the other Times are easily obtained. For the Number of Parts of the Plate just found being divided, namely that which answers to the horary Motion, in order to obtain the Motion of half an Hour, a Quarter of an Hour, and a Minute, a Table may be constructed only by Addition and Subtraction, in which having marked the Times, the Parts are put to the Plate, by which the Disk of the *Penumbræ* ought to be moved forward and backward, that it may receive the Situation accommodated to that Time. When this is done, it remains only to turn the Globe according to the Time, and the Plate in such a Manner, that it's Index may shew the Number ascribed to the Time.

Lastly the Places marked upon the Surface of the Globe, lying perpendicularly under the Disk of the *Penumbra* in any Situation of it, may be found by the Pendulum. But they are seen at one View, if the whole *Apparatus* is exposed to the Rays of the Sun reflected from a plain *Speculum*, in such a Manner, that the Rays may fall perpendicularly upon the Horizon of the Globe. For then such Shadows will be projected from the Disk of the *Penumbræ* upon the Globe, as are like the *Penumbræ* which the Moon casts upon the Earth, by which the Phases of the Eclipse, for any Place may be seen.

This Motion of the Sun is inconvenient; perhaps those who have a large burning Glass, will make Use of a Lamp, the Rays of which may be thrown upon the Globe from the Glass, in a Position perpendicular to it's Horizon. I have thought also of viewing the Globe from a Distance thro' a Perspective Glass, by which Method the Disk *k l m*, being brought upon the Surface of the Globe, exhibits the *Penumbra*. But this requires a very large Telescope; for if the Globe is set at such a Distance, that the whole may be seen thro' a small Telescope, I am afraid the Places marked upon it will not be distinguishable.

I have thought also of giving a Motion to the Machine, by means of two separate Clocks, one of which might turn the Globe, and the other the Plate; and these might be brought to agree exactly by the Help of Pendulums. But I have said enough already on this Subject.

XI. 1. The Observations were made with a Telescope of 10 Palms. True Time.

Eclipse of the Moon, Nov. 20, 1732. *observed at* Rome *by the Abbots* Didacus Revillas, *and* Jo. Bottarius, *and by* Eustachius Manfredi. No. 428. p. 85. April, &c. 1733.

h	′	″	p. m.
8	45	28	The Penumbra begins to be sensible.
	49	14	The Penumbra thicker.
	51	19	Beginning of the Eclipse.
		44	Grimaldus begins to immerge.
	52	47	All Grimaldus hid.
		54	Galilæus.
	53	48	The Shadow at Gassendus.
	56	2	All Gassendus hid.
	57	23	Schikardus.

True Time			
h	′	″	p. m.
9	2	43	Kepler.
	4	53	All Ariſtarchus hid.
	5	0	Lanſbergius, and almoſt all the Mare Humorum hid.
	6	13	Bullialdus.
		53	Capnanus.
	7	8	The Shadow at Mare Nubium.
	8	2	Copernicus begins to immerge.
		29	Thro' the Middle of Copernicus.
	10	27	The Shadow at Eratoſthenes; and all Copernicus hid.
	14	12	Tycho begins to immerge.
		45	*Inſula Sinus medii.*
	15	37	Heraclides.
	16	22	Tycho hid.
	18	12	Tymocharis.
	20	4	Archimedes.
	21	4	Harpalus.
	23	10	Manilius.
		16	Helicon.
		40	Plato.
	26	21	Menelaus.
	28	55	Catharina and Cyrillus.
	30	11	Pliny.
		56	Dionyſius.
	32	31	Ariſtotle.
	33	11	Promontorium acutum.
	34	27	Fernelius.
	34	51	Snellius.
	36	11	Poſſidonius.
		41	Petavius.
	37	45	Promontorium Somnii.
	38	25	Langrenus.
	40	24	Hermes.
	41	0	Proclus.
		30	Mare Criſium begins.
	42	32	Cleomedes.
	45	10	The Shadow thro' the Middle of *Mare Criſium.*
	46	20	Meſſala.
	48	24	The total Immerſion.
0	57	5	Duration of the total Immerſion.
11	31	13	The Emerſion had without Doubt begun.
	33	13	Grimaldus had emerged.
	46	3	The Middle of Copernicus.
	51	17	Tycho.
		52	Plato.
	53	13	Archimedes.

True Time.

h	′	″	p. m.
11	56	36	*Insula Sinus Medii.*
	59	57	Eudoxus.
12	2	10	Manilius.
	3	26	Ariſtotle.
	4	25	Menelaus.
	8	11	Poſſidonius.
	13	6	Pliny.
	17	14	Promontorium acutum.
	20	38	Langrenus.
	23	21	The whole *Mare Criſium.*
	26	55	End.
3	35	36	Duration of the whole Eclipſe.

Some Phaſes of the Immerſion taken by another Obſervation with a Newtonian Teleſcope.

True Time.

h	′	″	p. m.
8	50	13	The Penumbra thick.
	11	28	The certain Beginning of the Obſcuration.
	54	8	All Grimaldus hid.
		32	The Shadow thro' the Middle of Galilæus.
9	0	58	All Kepler hid.
	2	18	The Shadow at Ariſtarchus.
	3	37	All Ariſtarchus hid.
	8	3	The Shadow at the Beginning of Copernicus.
	9	20	Thro' the Middle of Copernicus.
	10	32	All Copernicus is covered.
	14	47	The Shadow at the Beginning of Tycho.
	23	11	At the Beginning of Manilius.
		26	At the Beginning of Plato.
		55	The Shadow thro' the Middle of Plato and Manilius.
	24	40	All Plato is covered.
	39	35	The Shadow at the Beginning of Proclus.
	40	18	The Shadow at Hermes.
	41	0	All Proclus is covered.
		31	At the Beginning of *Mare Criſium.*
	44	20	Thro' the Middle of *Mare Criſium.*
	46	15	All *Mare Criſium* is ſhaded.
	49	3	The total Immerſion of the Moon in the Shadow.

	h	′	″ apparent Time.	
2. The Beginning at — — — —	8	1	30	— *Obſerved in* Fleet-ſtreet, London, *by Mr.* Geo. Graham, *F. R. S.*
Immerſion — — — — —	8	59	30	
Emerſion — — — — —	10	38	0	
End — — — — — —	11	37	0	Ibid. p. 88.

Obſerved with a ſmall Teleſcope about 18 Inches long, which magnifies about 13 times.

N. B. Mr *Hodgſon* at *Chriſt*'s-*Hoſpital*, with a 4 Foot Teleſcope, obſerved the Beginning at $8^h\ 1'\frac{1}{2}$ and the End $11^h\ 36'\frac{1}{2}$.

Eclipſe of the Moon, Oct. 2, 1732. Styl. Nov. *obſerved at* Wittemberg *in* Saxony, *by* Jo. Frider. Weidler, *F. R. S.* No. 443. p. 359. Oct. 1736.

XII.

h	′	″	*Temp-europ. ante Mer.*
0	44	30	The Penumbra near Schikard.
	59	0	Beginning of the Eclipſe.
1	1	30	The Shadow touches Schikard. The Edge of it is rough and unequal. Soon after the Clouds hide the Moon.
1	15	0	Tycho is quite ſhaded. The Moon again covered with Clouds.
1	25	30	The obſcured Portion of the Moon is blackened, and the Spots cannot be diſcerned thro' the Shade by a Teleſcope of 9 Feet.
1	30	0	The Shadow touches Grimaldus. Now the Spots are ſeen thro' the Shadow.
1	44	30	The Shadow covers all Grimaldus. Now the ſhaded Portion is red. The Moon is again covered with Clouds.
2	25	30	The Shadow receding touches Lanſbergius. It's Edge is ſtill rough.
2	44	0	The Shadow touches Gaſſendus.
3	11	0	Tycho begins to emerge.
3	36	0	The End upon Snellius, the Sky being clear round the Moon.

Eclipſe of the Moon obſerved by Mr Geo. Graham *in* Fleetſtreet, March 15, 1735-6. No. 445. p. 14. Jan. &c. 1737.

XIII. 1.

h	′	″	
10	13	0	The Beginning.
11	11	0	The total Immerſion.
12	49	0	The Emerſion.
13	47	0	The End.

— *Obſerved by Dr* Halley *at* Greenwich, Ibid.

	h	′	″
2. The Beginning — — — — — — — —	10	13	37
The Immerſion. — — — — — — —	11	9	42

— *Obſerved at Mr* Graham'*s houſe in* Fleetſtreet, *by Mr* Celſius. Ibid.

3. The Obſervation was made with a reflecting Teleſcope of 4 Inches, made at *Edinburgh*, and magnifying 63 times.

h	′	″	
10	22	5	The Shade on the Middle of Kepler.
	23	15	Entering the Mare Humorum.
	28	16	Entering on Copernicus.

Entering

h	′	″	
10	29	34	Entering the Middle of Copernicus.
	30	26	Copernicus entire.
	33	28	Enters on Timocharis.
	38	44	Enters on Tycho.
	39	12	The Middle of Tycho.
	40	48	Tycho entire.
	46	0	Enters on Menelaus.
	49	20	Plinius.
11	0	40	Enters on *Mare Crisium.*
	5	36	*Mare Crisium* entire.
	9	17	The total Immersion is about to begin.
13	13	55	Tycho is emerged out of the Shade.
	29	0	Mare Serenitatis is totally emerged.
	40	45	*Mare Crisium* is totally emerged.
	45	50	The Eclipse is nearly ended.
	46	12	The Eclipse is certainly ended.

4.

— *Observed in* Covent-Garden, *by* J. Bevis, *M. D.* Ibid. p. 16. Fig. 73.

True Time.

h	′	″	p. m.
6	53	47	Saturn in the Point where the Threads of the Micrometer cross.
7	31	5	First of the Hyades at ♉ passes the Thread *a.*
7	31	50	It passes over the Thread *b.*
7	32	35	It passes over the Thread *c.*
7	42	39	Saturn again in the Intersection of the Threads.
8	19	57½	First of the Hyades at ♉ passes the Thread *a.*
8	20	42½	It passes the Thread *b.*
8	21	27½	It passes the Thread *c.*
9	50	0	The Disk of the Moon runs over the horary Thread, 139 horary Seconds.
9	56	0	Again 139″.
10	1	0	Again 139″.
10	9	40	A thin Penumbra seems to cloud the Moon near Hevelius.
10	10	20	Now very sensible.
10	11	40	I reckon the beginning of the Eclipse.
10	14	38	The Edge of the Shadow, as I think, passes thro' Grimaldus and Cavalerius.
10	19	46	Thro' Aristarchus.
10	24	15	The Shade enters the Mare Humorum.
10	32	44	It touches the Sinus Roris.
10	40	18	The Shade divides Tycho.
10	42	26	It touches the Mare Serenitatis.
10	46	1	It touches Menelaus, a black Cloud comes over.

On

True Time.

h ′ ″	p. m.
10 53 46	On the Departure of the Cloud the whole Mare Nectaris was found covered. Very thick Clouds obscure the Moon again.
11 0 56	The Shadow touches *Mare Crisium.*
11 5 48	*Mare Crisium* and *Mare fœcundum* are immerged.
11 10 0	The total Immersion of the Moon into the Shadow.
12 42 20	The eastern Limb of the Moon grows clear.
12 46 5	It grows still more clear.
12 47 56	A Thread of pure Light is restored in the twinkling of an Eye. Many light Clouds.
12 57 5	The Edge of the Light touches the Mare Humorum.
13 4 3	The whole Mare Humorum is recovered.
13 13 40	Tycho is half covered.
13 14 0	Quite uncovered.
13 17 22	Waltherus emerges. Many dark Clouds, which seem likely to last some Time.
13 43 44	Mare fœcundum is seen out of the Shadow.
13 46 25	The true Shadow ends.
13 48 30	The *Penumbra* no longer sensible.

In these Observations I made Use of a very good Clock, corrected by 5 corresponding Altitudes of the Sun this very Day, and several Days before, and a Telescope 6 Feet long. About the middle of the Obscuration, the Moon appeared as thro' a darkish Cloud, but at the Edges it was red like hot Iron. The Limit of the Light and Shade was not well determined thro' the whole Eclipse.

— *Observed at* Yeovil *in* Somersetshire, *by Mr.* John Milner.

5. The Latitude of *Yeovil* is 50° 52′. The Clock was first adjusted by the Equation Table.

	h	′	″	dig.	′
The Beginning — — — — — — —	10	6	0		
The Moon's Altitude then — — — — —				34	29
The Beginning of total Observation — —	11	4	30		
The Altitude then was — — — — —				34	16
The Middle — — — — — — —	11	54	0		
The End of total Obscuration — — — —	12	43	30		
The Altitude then was — — — — —				34	47
The End — — — — — — — —	1	39	15		
The Altitude then was — — — — —				31	47
The Continuation of the total Obscuration was —	1	39	0		
The Duration of the whole Eclipse — — —	3	33	15		

Pla XII. Vol. VIII. part 1. pa. 166.

Fig. 70.

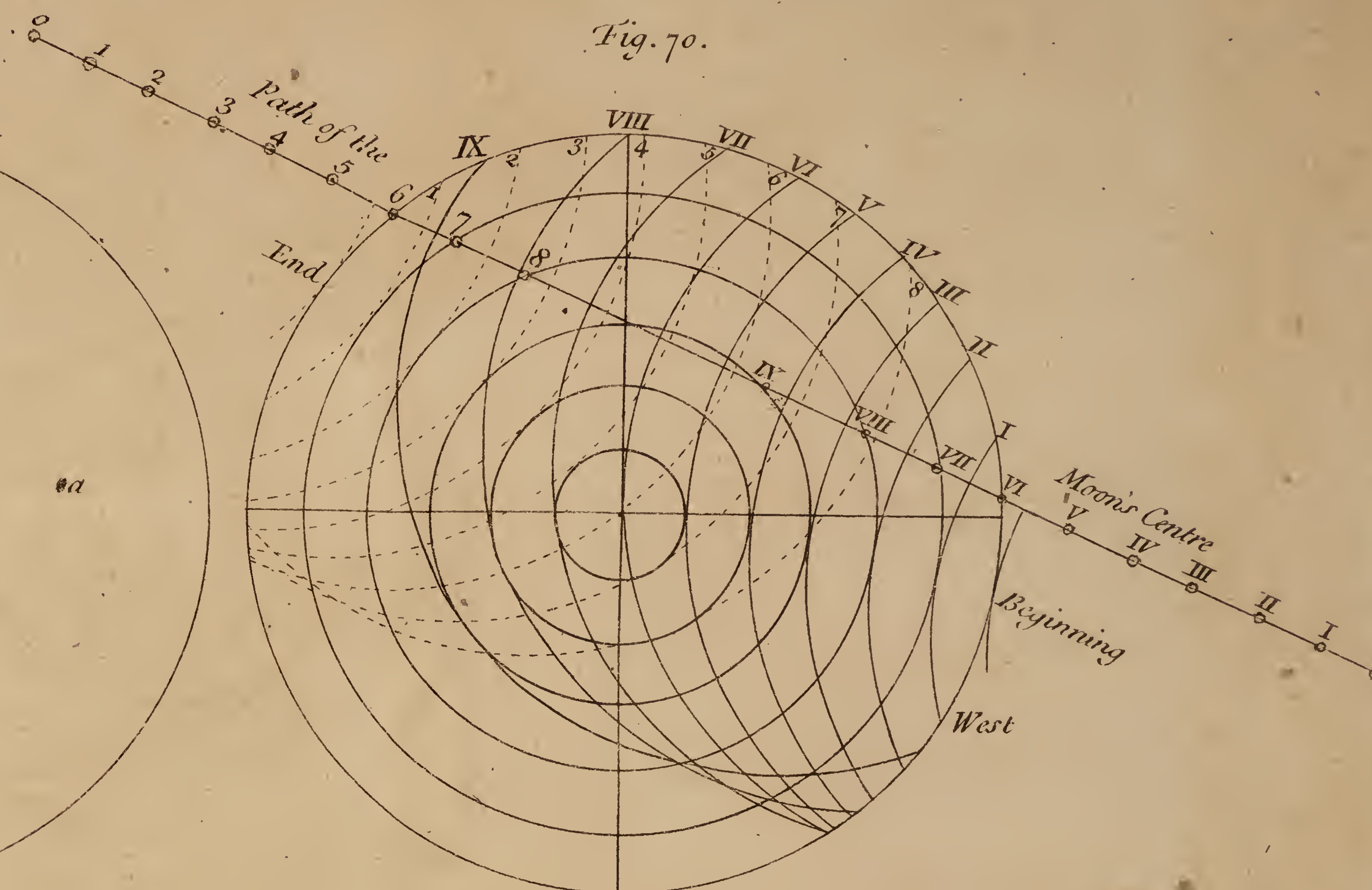


Fig. 71.

Fig. 72.

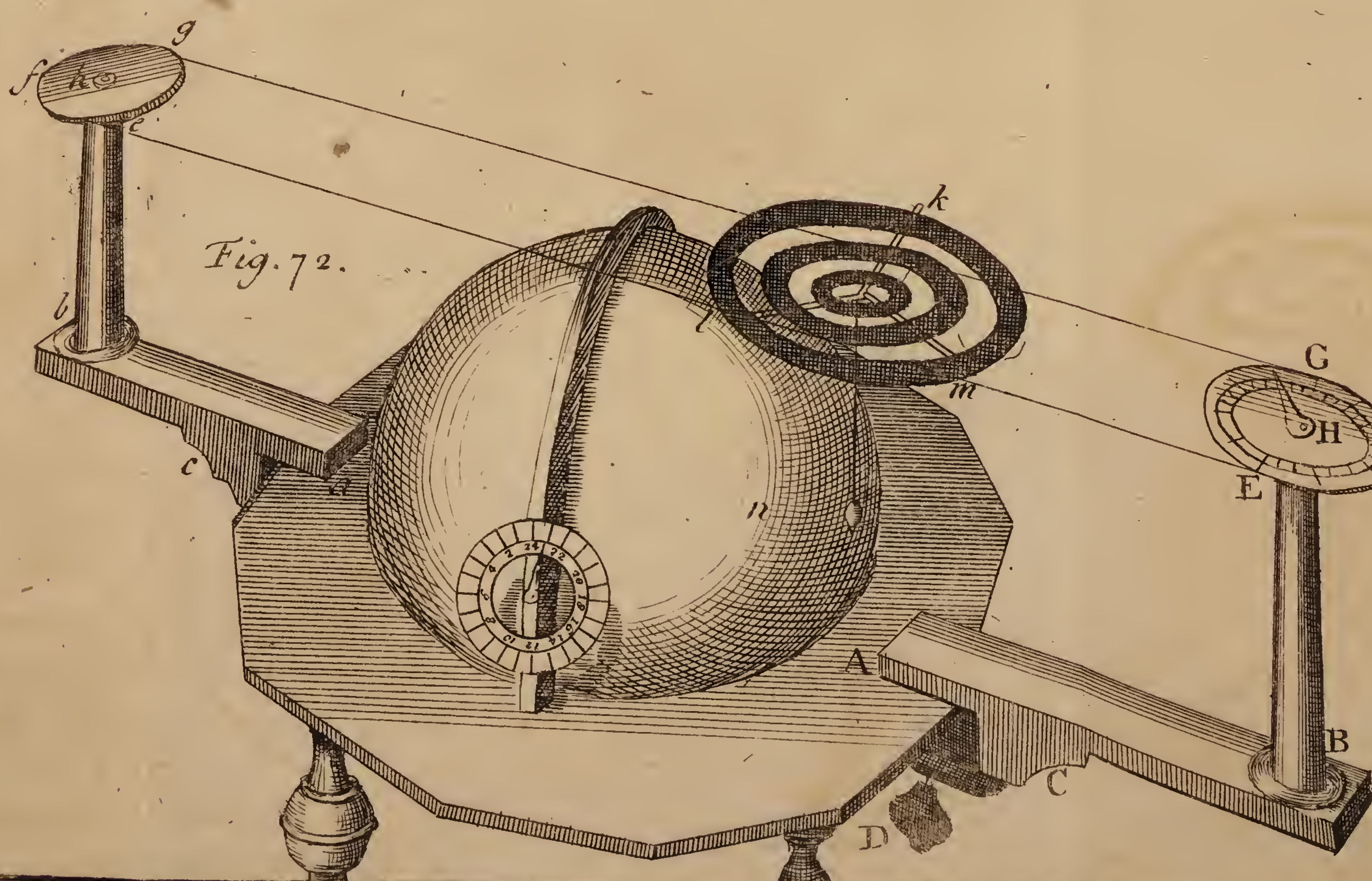
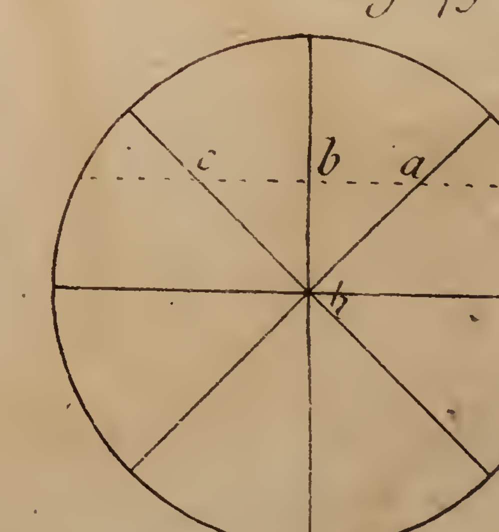


Fig. 73.



The Telescope inverting the Object.

XIV. 1.

Eclipse of the Moon, Sept. 8. 1736, *observed in* Fleetstreet, London, *by Mr* Geo. Graham, *F. R. S.* *and by Mr.* James Short *of* Edinburgh, *F. R. S.* No. 466. p. 92.

	h	′	″	Appar. Time.
Beginning of the Eclipse — — — —	12	58	0	
The Shadow touched Grimaldi — — —	13	0	0	
touched Kepler — — —		9	30	
touched Copernicus — —		17	10	
touched the East Side of Tycho		25	5	
touched the East Side of Plato		34	30	
touched the East Side of Manilius		36	40	
touched the E. S. of *Mare Crisium*		56	20	
Beginning of total Darkness — — —	14	3	45	

The Observation made with a 5½ Inches reflecting Telescope, magnifying about 38 times.

— *In* Covent-Garden, London, *by* J. Bevis, *M. D.* Ibid.

2. The Observation was made with a Telescope of 5 Feet.

Apparent Time.

h	′	″	
12	53	25	The Penumbra touches the North East Limb. Clear.
	54	25	Now very conspicuous. Clear.
	56	50	The true Shadow, as I judge, touches the Limb. Clear.
	57	30	The Shadow touches Grimaldi. Clear.
13	0	25	Grimaldus covered. Clear.
	7	23	It enters the Mare Humorum thro' thin Clouds. Clear.
	28	39	The Shadow touches the Mare Vaporum. Clear.
	31	19	The dark Part of the Moon is of a reddish Colour. Very clear.
	36	53	The Limit of the Shadow bisects Manilius and touches Mare Serenitatis. Very clear.
	38	48	It touches Mare Tranquillitatis. Clear.
	47	21	Mare Serenitatis is covered. Clear.
	55	26	It touches *Mare Crisium*. Clear.
	58	5	Mare fœcunditatis is covered.
14	2	25	Total Immersion of the Moon. Very thick Clouds come over and hide the Moon.
16	43	0	Mare Tranquillitatis seems quite uncovered thro' a Gap of the Clouds.
	43	30	Clouds again.
17	3	22	The Cloud going off, the Moon seems to be free from all Obscurity.

The Clock was fitted to true Time by equal Altitudes of the Sun; and it's Agreement with Mr *Graham*'s Chronometer was marked by a very good Watch.

——*At* Wittemberg, *obſerved by* J. Frederick Weidler. *F. R. S.* Ibid. p. 94.

3.

h	′	″	
1	36	0	The Penumbra comes upon the Eaſt Part of the Moon, like a Miſt or Smoak.
1	50	0	Beginning.
1	50	30	The Shadow touches Grimaldi.
1	52	0	———————— Galilæus.
2	0	0	———————— Kepler.
2	1	30	———— covers all Kepler.
2	7	0	A Portion of the Lunar Diſk, immerged deeper into the Shadow, appears clearer than that which was nearer the Edge of the Shadow.
2	8	0	The Shadow touches Copernicus.
	10	50	———— covers all Copernicus.
	16	10	———— touches Tycho.
2.	20	0	Half of the Moon darkened.
	25	0	The Shadow touches Mare Serenitatis.
	29	10	———————— Menelaus.
	36	0	All Mare Serenitatis covered. The Moon looks red thro' the Shadow like a Coal of Fire.
	45	30	The Shadow at Mare Criſium. At this Time the Edge of the Shadow is bent inwards about Mare Criſium; and during the whole Eclipſe, the Circumference of the Shadow appeared rough and rugged, and ſeemed in the extreme Part to be ſurrounded with a Sort of light Smoak.
2	50	0	All Mare Criſium ſhaded.
	53	0	Total Darkneſs, Now about ⅓ of the Lunar Diſk towards the Eaſt appears darker than the Weſt part.
3	43	0	The Shadow darker in the Middle, but paler about the extremities.
4	8	0	The Moon is covered with Clouds.
	44	0	Emerſion of the Moon out of the Shadow.
	45	0	The Shadow leaves Grimaldi. After this the Moon was hid by Clouds, out of which it now and then emerged, but a Miſt or thin Cloud ſhaded it ſo, that the Spots could not be diſtinguiſhed. At Length the whole Moon was hid by thick Clouds.

The Obſervation was made with a Teleſcope, 8 Paris Feet long.

— *Obſerved in* Hudſon's-Bay, *by Capt.* Chriſtopher Middleton, *F. R. S.* Ibid. p. 96.

4. I made the Obſervation in *Hudſon's-Bay*, in Lat. 55° 34′ N. and on the Meridian of the *North-Bear Iſland*, which lies 30 Miles to the Weſtward of *Charlton*. The Weather was very clear, but the Motion of the Sea rendered my Teleſcope uſeleſs, and I miſſed the Beginning.

I

The

	h	′	
The total Immersion of the Moon's Body into the Shadow — — — — — —	8	22	by my Watch.
The Emersion — — — — — —	10	8	
The End — — — — — — — —	11	16	

In order to rectify my Watch, and be certain of the true Time, I took three several Altitudes next Morning, and one in the Afternoon, by Mr *Hadley*'s and Mr *Smith*'s Quadrants; which (having made proper Allowances for the Refraction of the Atmosphere and the Height that I stood above the Surface of the Sea) were as follows.

	°	′		h	′	
First Altitude	23	0	Hence the true Time is —	8	49	—
Latitude —	55	45	The Time by my Watch —	8	28	—
			Watch too slow — —	0	21	—
Second Altitude	25	48	The true Time therefore is —	9	15	—
Latitude —	55	45	The Time by my Watch —	8	54	—
			Watch too slow — —	0	21	—
Third Altitude	26	44	The true Time therefore is —	9	24	—
Latitude —	55	45	The Time by my Watch —	9	3	—
			Watch too slow — —	0	21	—
The fourth Altitude taken in the Afternoon the same Day —	21	29	Hence the true Time is — —	3	25	+
Latitude —	55	33	The Time by my Watch —	3	4	
			Watch too slow — —	0	21	+

If 21 Minutes therefore be added to the Times above-mentioned, for the Error of the Watch, we shall have the true Times of the several Observations on the Meridian of the *North-Bear Island*, as follows, *viz.*

	h	′
The total Immersion of the Moon's Body into the Shadow ——	8	43
The Emersion — — — — — — — — — — —	10	29
The End — — — — — — — — — — —	11	37

This same Eclipse was observed by Dr *Bevis* at *London*, and he made the true Time of the total Immersion of the Moon's Body into the Shadow, 14^h, $2'$, $25''$; consequently the Difference of Longitude between *London* and *North-Bear Island* in *Hudson*'s-*Bay*, is 5^h, $19'$, $25''$, or $79°$, $51'$.

An Eclipse *of the* Moon, Dec. 21, 1740. *at the Island of* St Catharine *on the Coast of* Brasil, *observed by the Hon.* Edw. Legge, *Esq; Captain of his Majesty's Ship the* Severn. No. 462. p. 18. Read Jan. 21, 174½.

XV. 1. It began very nearly at $7^h\ 5'$; but the Horizon being hazy, I could not observe exactly the Beginning: However, it ended exactly to a Moment, at $9^h\ 50'$. I set my Watch by two Observations before, that I might be exact in Time, and confirmed it by one after; so that I believe I may venture to say it was right: And I observed with one Telescope on board, and sent another on Shore, which agreed exactly together.

Remarks on the foregoing account by the Rev. Joseph Atwell, *D D. F. R. S.* Ibid.

2. The Captain places the Island in Lat. 27° 30′. Mr *Gael Morris* has calculated the said Eclipse; and the Middle of it, apparent Time, at *Greenwich*, was,

	h	′	″
	11	44	50
By the Captain's Observat. supposing the the Beginning exact	8	27	30
Difference of Meridian — — — — — — —	3	17	20
	= 49°20′		

	h	′	″
The End of it, by Calculation at *Greenwich* — — —	13	6	57
——————— by Capt. *Legge*'s Observation — — —	9	50	0
Difference of Meridian — — — — — — —	3	16	57
	= 49° 14′		

Capt. *Legge* observes, that in attempting to pass Cape *Horn*, they thought themselves to have been more to the Westward than they really were: By which Mistake, turning too soon to the North, they fell in with high Lands, and met with those Misfortunes, which, if they had kept out more at Sea, might probably have been avoided. By comparing the Longitude at *St Catharine*'s as above settled, with *Senex*'s Maps, the Coasts appear to be placed about 6 Degrees too much Eastward; and if the other Parts of *America* about the Cape are laid down as faultily in the Charts, this Error will probably account for their Misfortunes.

— *At* Cambridge *in* New-England, *by Mr* John Winthrop Hollisian, *Prof. Math. and Astron. at* Cambridge *in* New England. No. 471. p. 577. Read Nov. 3, 1743.

3. *Dec.* 21, 1740.

h	′	
5	24	A plain Penumbra.
	35	The true Shadow seems to enter.
	47	Touches *Palus Maræotis.*
	53	Reaches *Mount Sinai.*

After this the Clouds thickened, and covered the Moon till the End of the Eclipse, which was about 8^h, $30'$, as near as I could guess through the Clouds.

The

The Night before the Eclipse, *viz.* 20 *December*, at 12h, 14′, I saw the Moon eclipse a fixed Star, which, I think, is in the Heel of *Castor*.

These two Observations were made with an eight Foot Telescope, my Watch being rectified to the apparent Time by correspondent Altitudes of the Sun, taken with the before-mentioned Quadrant for several Days together, before and after the Eclipse.

An Eclipse *of the* Moon, Jan. 2, 1741. *observed at the College at* Pekin, *by the* Jesuits. No. 468. p. 309. Jan. 1742 3.

XVI. In the Morning.

h	′	″	
6	4	18	The Shadow was observed to have just touched the Edge of the Moon, with a Tube of 3½ Feet.
	5	40	The certain Beginning between *Vieta* and *Schikardus* with a Tube of 6 Feet.
	10	0	The Shadow at *Schikardus*.
	14	0	——————— *Mare Humorum.*
	16	0	——————— Grimaldi.
	16	10	——————— Capnanus.
	17	20	Gassendus begins to be immerged. The Centre of Grimaldi in the Shadow.
	18	30	All Grimaldi immerged.
	19	0	The Shadow at Campanus.
	19	30	——————— Herigonius.
	22	30	——————— Tycho.
	23	10	——————— Bullialdus.
	24	0	Tycho immerged.
	24	20	The Shadow at *Pitatus.*
	32	0	——————— Galilæus.
	42	0	——————— Kepler.
	43	0	Reinholdus.
	55	0	Fracastorious is immerged. The Shadow at Copernicus
7	2	0	All Copernicus seems to be in the Shade.
	6	0	The Shadow at Wendelinus.
	10	20	The Centre of the Moon in the Shade.
	14	0	The Moon is hid behind Mountains at the Setting, before the Middle of the Eclipse.

The *Penumbra* was small and the Shadow very black and distinct, and the Eclipse might very plainly and clearly be seen 'till the Setting of the Moon. The Diameter of the Moon before the Eclipse was in the Micrometer about 30′ 20″, but at the Setting only 30′ 00″. The Centre of the apparent Disk was from the *Sinus Æstuum Occid.* a little towards *Hipparchus.*

An Eclipse *of the* Moon, Jan. 2, 1740. *observed at Mr* Graham's *House in* Fleet-street, *by Mr* Short. No. 459. p. 633. Jan. *&c.* 1741.

XVII. The Observation was made with a reflecting Telescope of 9 Inches Focus, that magnified about 40 times.

	h	′	″	
Beginning about — — — — — —	8	25	00	Time appar.
Beginning of total Darkness at — — ——	9	31	10	
End of total Darkness — — — — —	11	15	20	
End of the Eclipse at — — — — —	12	22	00	

N. B. The Beginning and End could not be distinctly seen for Clouds.

Eclipse of the Moon, Oct. 22, 1743, *in the Morning, observed at Mr* Graham's *House in* Fleet-street. No. 471. p. 580. Read Nov. 3, 1743.

XVIII. The Sky was mostly overcast with Clouds, so that the following Observations are the only ones that could be made with any Degree of Certainty.

	h	′	″
Beginning of the Eclipse about — — — — —	1	21	0
The Shade touched Copernicus about — — — —	1	39	0
touched Plato about — — — — ——	1	45	0
touched Tycho about — — — — —	1	51	0
Total Immersion about — — — — — ——	2	17	0

Of the Lunar Atmosphere, by M. Jean Paul Grandjean de Fouchy, *of the R. Acad. at* Paris. No. 455. p. 261. Nov. *&c.* 1739.

XIX. By the Name of Atmosphere is understood, a certain Congeries of pellucid Matter involving a Planet, and capable of turning the Rays of Light, that pass thro' it, from a right Line; whether this Matter exists in our Air jointly, or separately from it, whatsoever it is, we treat here only of the refracting Matter, and that is what I only take upon me to prove in the Course of this Work, that there is no Matter about the Moon, which is able to turn the Rays of Light sensibly from their strait Course. I would inform the Reader only of this, that I here conceive the Atmosphere to be a homogeneous Fluid, with a spherical Surface, and of the same Density every where, which is equal to the Sum of the decreasing Densities in the real Atmosphere, purposely omitting the Difference of the Density of it's Parts, which cannot disturb our Demonstrations.

Fig. 74.

Now if the Moon is encompassed with an Atmosphere, it's Diameter ought to be found greater than in the naked Planet; but that the Quantity of it's Increase may be known, let A I B be the Body of the Moon, G E F it's Atmosphere, the Angle A H L will be the real Diameter of the Moon; and the Angle E H L comprehended under the *Axis* L H, and the Ray A E H will be the Diameter of the Moon observed. Therefore the Angle E H A will be the Increase of the Diameter of the Moon by it's Atmosphere, but the Angle E H A is opposite to the Side E A of the Triangle E H A; and the Angle A E H the Supplement to 180 of the horizontal

rizontal Refraction in the Lunar Atmosphere, is opposite to the Side A H of the Distance of the Moon from the Earth. Moreover the Side E A, is the Half of a Chord of the Lunar Atmosphere, touching the Body of the Moon itself in A. Therefore the Sine of the Increase E A H of the Diameter of the Moon by it's Atmosphere, will be to the Sine of the Supplement of the horizontal Refraction A E H, as the Half A E of the Chord of the Atmosphere touching the Body of the Moon at the Distance A H of the Moon from the Earth.

Hence it follows evidently, that this Increase of the Lunar Diameter is insensible; for if it arose to $2\frac{1}{2}''$, supposing the horizontal Refraction $5'$, that is, at least 30 Times greater than it can be supposed, as will be proved hereafter, then the Semichord E A would equal 276 French Leagues, and would far exceed a like Chord of the terrestrial Atmosphere. Therefore whether the Moon is covered with an Atmosphere or not, it's Diameter will always be observed the same; and the Observation of the Lunar Diameter can by no Means be equal to the Solution of the Question.

The Eclipses of the Sun by the Moon, give a greater Handle for deciding the Doubt; for the extreme Rays terminating the Cone of the Lunar Shade, as they touch the Body of the Moon, and pass thro' it's Atmosphere, will necessarily be inflected toward the *Axis* of the Cone; therefore the Cone will become shorter and more obtuse; but to shew the Quantity of that Variation, we must observe, that the Ray F A, or it's Parallel E G, which, if there was no Atmosphere, would be the Bound of the Lunar Shade F A C, would be refracted toward the *Axis* C A, at the Ingress of the Atmosphere G, and at the Egress H; whence the Semiangle of the Cone of the Lunar Shade will be increased by double the horizontal Refraction in the Lunar Atmosphere. Fig. 75.

Hence it follows, that if we suppose a lunar Atmosphere, a total Eclipse of the Sun will begin later and end sooner, than if we do not suppose it; moreover, in certain Cases, that there will be no total Eclipse; which however the Diameters of the Sun and Moon observed in the same Degreee of Anomaly would require; for in these Cases the Cone of the Lunar Shade is contracted, because of the Atmosphere, and it might be so contracted as not to touch the Disk of the Earth even with it's Point.

After the same Manner exactly, the Duration and Quantity of the partial Eclipses would be diminished; for the Beginning of a partial Eclipse is observed, when the Cone of the Penumbra G D I enters upon the Habitation of the Observer; but a double Refraction F C E, E V H being supposed in the Atmosphere of the Moon, the Semiangle of the Cone of the Penumbra is diminished, and the Semidiameter of the Base G I is contracted into I H; therefore that the Beginning of an Eclipse may be observed in a given Fig. 76.

Place, a Space G H equal to the Centre I of the *Penumbra*, muſt be run over; the ſame muſt be ſaid of the Emerſion. Therefore a partial Eclipſe will begin later and end ſooner, ſuppoſing a Lunar Atmoſphere, than if the Moon is naked; and it will alſo be obſerved to be leſs, for the Habitation T being immerged into the *Penumbra* by the Quantity T N, ſuppoſing a Lunar Atmoſphere, will enter it only by the Diſtance T K. It may alſo be, that no Eclipſe may be obſerved in that Place, where it would be obſerved, if no Atmoſphere is ſuppoſed to be about the Moon; for the Diſk of the *Penumbra* being altered, the Place R, which if the Moon is naked, would be immerged into it, will become free from it by the Quantity R N. But they who ſhall live in the Space Y H, comprehended between the direct Ray X Y touching the Atmoſphere, and the refracted Ray E H, terminating the *Penumbra*, will ſee the Sun free indeed from the Body of the Moon, but obſcured by it's Atmoſphere; and therefore a certain pale *Penumbra*, which, by what has been already demonſtrated, muſt precede and follow the Diſk of the Moon; moreover this Obſcuration may be obſerved without any Eclipſe.

Theſe *Phænomena* muſt principally be obſerved in the Solar Eclipſes, if there is any Atmoſphere about the Moon; now let us ſee what is really obſerved.

In the firſt Place, as the *Axis* of the Lunar Shade is extended to 55 Semidiameters of the Earth, when greateſt, and to 52½ when it is leaſt, and as the leaſt Diſtance of the Moon from the Earth is 54 Semidiameters of the Earth, if the lunar Atmoſphere was capable of a horizontal Refraction of 8″, the Semiangle of the ſhady Cone will be increaſed by double the Quantity, that is, 16′, and therefore it will be equal to 16′ 41″, when it is moſt open, and to 16′ 5″ when it is narroweſt. Moreover the leaſt Semiangle of the Cone being ſuppoſed equal to 16′ 5″; it's *Axis* will be leſs than the leaſt Diſtance of the Moon from the Earth, of 54 Semidiameters of the Earth, and therefore the Point of the Lunar Shade, will reach to the Earth. If therefore there is an Atmoſphere about the Moon, in which the horizontal Refraction is 8″, there will be no total Eclipſe upon the Earth. Therefore either there is no Atmoſphere about the Moon, or if there is any, it produces a horizontal Refraction leſs than 8″.

But there are total Eclipſes of the Sun obſerved with a Duration of the total Darkneſs. For Inſtance, in the Eclipſe of 1724, the the Duration of the total Darkneſs amounted to 2′ 16″. The Moon at that Time ran over 1′ 52″ in it's horary Motion, and it's Shade always parallel to it in Degrees of the Diſk of the Earth went over a Space 54 Times greater, that is, equal to 1° 7′ 30″; from which, if we take away the diurnal Motion of a Habitation equal to 20′, which may prolong the Duration of the Eclipſe, we ſhall

ſhall have the Diameter of the Shadow equal to $47' \ 30''$, or 45173 Toiſes, or 22 Paris Leagues. Whence, by Calculation, we find that the *Axis* of the Cone of the Lunar Shade, is greater by at leaſt one Diameter of the Earth, than the Diſtance of the Moon from the Earth, which was then the leaſt, the Moon being about the *Perigæum.* Moreover from the given Diameters of the Luminaries, obſerved in the ſame Degree of Anomaly, the *Axis* of the Cone of the Lunar Shade is found to be equal at leaſt to 55 Semidiameters: Whence it follows, that the Spot of the Lunar Shade on the Diſk of the Earth, and the *Axis* of the Cone are found to be exactly the ſame, as the Diſtances of the Moon and the obſerved Diameters of the Lunaries ſeems to require. There is therefore no Atmoſphere about the Moon, or, if there is any, it cannot produce any ſenſible Refraction. But that there may be no Room left for Doubt, I ſhall give a Reaſon for thoſe *Phænomena*, which being obſerved in the Solar Eclipſes, have given Room to imagine a Lunar Atmoſphere.

Firſt indeed, that diminutive Light, which is obſerved in total Eclipſes, does not prove any Refraction in the Fluid, which encompaſſes the Moon; for by *M. Maraldy*'s Experiments, which have been repeated by me with the greateſt Care, and with the ſame Succeſs, it is manifeſt, that the Shadow of Bodies not covered with any Atmoſphere, if they are expoſed to the Sun, are bright about the *Axis* of the Cone; and the more ſo as it is the farther from the Body itſelf. Moreover the Habitation of the Obſerver in a total Eclipſe is about the *Axis* of the Cone of the Lunar Shade, and in the Neighbourhood of it's Point. It is no wonder, therefore, that the Middle of the Shadow is covered with a malignant Kind of Light, which may otherwiſe be increaſed by the Rays being reflected by an illuminated Air ſurrounding the Shadow about the Middle.

Secondly, the lucid *Annulus* ſurrounding the Moon in total Eclipſes, by no Means proves the Exiſtence of the Lunar Atmoſphere, as will appear to any one that hides the Sun from him by Balls of Wood or any opake Matter. Wherefore it is to be aſcribed not to a Lunar, but a Solar Atmoſphere, as has abundantly been proved, by *M. Mairan* *, in his Treatiſe of the *Aurora Borealis.*

Thirdly, the Diminution of the lunar Diameter, which in the Solar Eclipſes is obſerved to be about $30''$ leſs than when the Moon ſhines with a full Orb in the ſame Degree of Anomaly, by no means proves the Lunar Atmoſphere; tho' ſome Inequalities of Mountains are obſerved in the Circumference of the Diſk of the Moon, which quite diſappear in the Full-Moon; for lucid Objects ſtrike the Fibres of the Eye ſo ſtrongly, that the Motion of them is communicated to the neighbouring Fibres, and ſo the Image of the lucid Body is increaſed beyond the due Quantity, which is known by common Experience; for if a Stick is placed between the Moon and the

* Sect. I. cap. i. pag. 14.

the Eye, the Diameter of the Stick over againſt the Moon will ſeem to be diminiſhed; but if at that Time any Cloud comes over the Luminary, the Diminution of the Stick will appear leſs, but if the Cloud take away the Sight of it there will be no Diminution; and laſtly it will be various according to the various Intenſeneſs of the lunar Light.

As for the Inequalities of the Mountains, they are leaſt obſerved for the ſame Reaſon in the Full-Moon; for the lunar Mountains obſcure of themſelves, and ſeen in the bright Orb of the Sun, eſcape the Eye much leſs, than when ſhining in the Full-Moon, they are extinguiſhed in the neighbouring Splendor of that Luminary; eſpecially as the lunar Light is ſo intenſe, that a Star of the third Magnitude can hardly be ſeen when near it. But, to take away all Doubt in this Affair, if the Limb of the Moon oppoſed to the Sun was the Bound of it's Atmoſphere, and not of it's very Body, the Mountains in the Circumference of the Moon would never be obſerved by the longer Teleſcopes with narrower objective Apertures. I have often obſerved ſeveral Inequalities of Mountains in the Diſk of the Full-Moon with a Teleſcope of 36 Paris Feet, and an objective Aperture of one Inch; whence it follows, that the Diſk of the Full-Moon is terminated by the Circumference of it's Body and not of it's Atmoſphere.

This Obſervation was really made by Dr Halley *in the Preſence of* M. Delouville. *See* Vol. IV. p. 258, 259.

Fourthly, I muſt ſpeak a little of that wonderful Obſervation, in 1715, of the lunar Coruſcations, which was made by *M. Delouville*, in the Preſence of many Aſtronomers of the Royal Society. We may ſuppoſe, that the viſible Limb of the Moon is compoſed of the Tops of Mountains; which, in a total Eclipſe, hide the Sun from the Obſerver in the ſame Manner as the Trees of great Woods obſtruct the Sight. Whence if ſome Rows of Mountains on the Surface of the Moon afford a free Paſſage in a right Line to the ſolar Rays, they muſt imitate a Sort of Coruſcations, in the ſame Manner, as when in a *Camera Obſcura*, a Ray of the Sun by Means of a *Speculum* is ſuddenly admitted, and the Picture of external Objects drawn on the *Focus* of the *Lens* is taken away, it will be illuminated with luminous Tracts very much reſembling Lightning; which I think is the more eaſily to be allowed, becauſe thoſe ſudden Coruſcations have always been obſerved near the Limb of the Moon, as appears from the Scheme of this Eclipſe in Sir *Hans Sloane*'s *Muſeum*, drawn by his Daughter.

As for that pale Ring accompanying the Limb of the Moon in this Eclipſe, as nothing like it appeared either to me, or to any other Aſtronomer in the Solar Eclipſes hitherto obſerved, which however, according to the Hypotheſis of the Lunar Atmoſphere, muſt always and every where be obſerved, we ſhall make no Mention of it here.

From

From all this it is manifeft, that there is nothing like a Lunar Atmofphere in the Eclipfes of the Sun. I fhall now fpeak of the Eclipfes of the Fixed Stars and Planets by the Moon.

If the Moon is furrounded by an Atmofphere, the Planets and fixed Stars will be feen by an Obferver placed on the Surface of the Earth, to be hid later behind the Moon, and to emerge fooner from it's Difk, than if the Moon is fuppofed to have no Atmofphere; nay, and in fome Places, where an Eclipfe of a fixed Star or a Planet ought to be feen, there will be none. To make this plain, let A B C be the Body of the Moon, and let a Star be placed as it were at an infinite Diftance in S; the parallel Rays L V, M X, touching the Body of the Moon on all Sides, conftitute a cylindrical Surface, of which Cylinder the Bafe V Z X comprehends in it's Compafs all the Habitations on the Difk of the Earth, in which the Star or Planet is covered by the Moon. The Obferver therefore will fee the Beginning of the Eclipfe at V, and the End of it at X, and will meafure the Duration of the Time, in which the Moon may run thro' it's Diameter, or rather a Space equal to it. But if we fuppofe an Atmofphere of the Moon, the Ray I W will not remain parallel to the *Axis* of the Cylinder, and the Cylinder itfelf will become a Cone, of which the Section Y T U will mark the Habitations where the Eclipfe muft be. And the Bafe Y T U being contracted, the Point Y will come upon the Habitation later than the Point V; and the Limit U will forfake it fooner than X: Therefore, the Eclipfe of a Star or Planet by the Moon will begin later and end fooner, if we fuppofe an Atmofphere about the Moon, than if there is none: And there will be no Eclipfe in that Place where it ought to be obferved without an Atmofphere; for the Place C being covered by the Circumference V Z X of the former Cylinder, will be free from the Section of a Cone Y T U. Befides, fuppofing the horizontal Refraction in the Atmofphere of the Moon equal to 8'', V Y will be equal to 1384 Toifes, or $\frac{2}{7}$ of a Paris League; whence it follows, that no Eclipfe muft be obferved in the Places pointed out in the Calculation, as often as they are immerged into a cylindrical Area not exceeding $\frac{2}{7}$ of a League. Fig. 77.

Another *Phænomenon* alfo arifes from the Suppofition of a Lunar Atmofphere; in the Part of the Cylinder Y R, the Star indeed will always be feen, but thro' the Interpofition of the Lunar Atmofphere; and therefore it will acquire a different Motion and Colour from the true; and that in all Eclipfes whatfoever, whether the Star is one of the biggeft or leaft.

Befides, the Duration of the Eclipfes of the Fixed Stars and Planets by the Moon, does not feem in any Manner diminifhed, but is always found to be exactly agreeable to the Diameter of the Moon and it's Motion. As for thofe Obfervations, in which the Star after the Contact is feen to proceed a little in the Difk of the Moon before the Occultation, we fhall refer the whole Caufe of them to the in-

creased Diameter of the Moon and Star; for if the Lunar Atmosphere was the Cause of this Appearance, it would always be observed the same in all Stars, and in any Apertures of Objectives. Besides, I have not as yet observed the Progression of any Star in the Disk of the Moon, unless it was of the first, or at least of the second Magnitude, and that by half of it at most, and the true Diameter of the Fixed Stars, as is manifest to any Observer, becomes insensible, and is increased only by spurious Rays; whence the adventitious Rays both of the Star and the Moon, are mixed in the Bottom of the Eye before the true Conjunction of the Bodies of the Sun and of the Moon; and if the visible Limb of the Moon was the Bound of the Atmosphere, and not of the Body, no Mountains would be observed on it's Circumference with greater Tubes and narrower objective Apertures; which however, as has been said, are seen plainly enough.

From all this it is manifest, therefore, that the Moon is not surrounded with a refracting Atmosphere, the Refraction of which is capable of being observed; for there might be an Atmosphere about the Moon, in which the horizontal Refraction amounted to 1″ or 2″; for this Opinion seems to be countenanced by the greater Spots in the Moon, which cannot by any Means be taken for Woods, as *Hartsoeker* and others have imagined. For the Shadows of the Edges are always observed nearer to the bright Limb of the Moon; whence it is rightly concluded, that they are Cavities and not Woods, which would project a Shadow from the other Side. Moreover some Fluid may be supposed to be in them, in which case it would be very agreeable to Philosophy, that some Vapours should be raised from them, the Congeries of which would represent a Sort of an Atmosphere about the Moon, which Atmosphere would not be found to be very thick; for by Sir *I. Newton*'s Demonstrations, it would hardly equal $\frac{1}{3}$ of the Density of the terrestrial Vapours, nor would be alike at different Times, those Vapours being destitute of any other Addition.

A Conjunction of Saturn *and* Mars, *observed at* Wittemberg *by* Joh. Frid. Weidler, *F. R. S.* No. 441. p. 238. April, &c. 1736.

XX. Saturn and Mars were seen *Feb.* 5. 7^h 30′ p. m. in the same right Line with the Star E ♓ Bayeri.

E ♓
*

♂ o

♄ o

♄ ♂ 2° 15′
♂ E ♓ 1 12 30″

Feb.

Fig. 74.

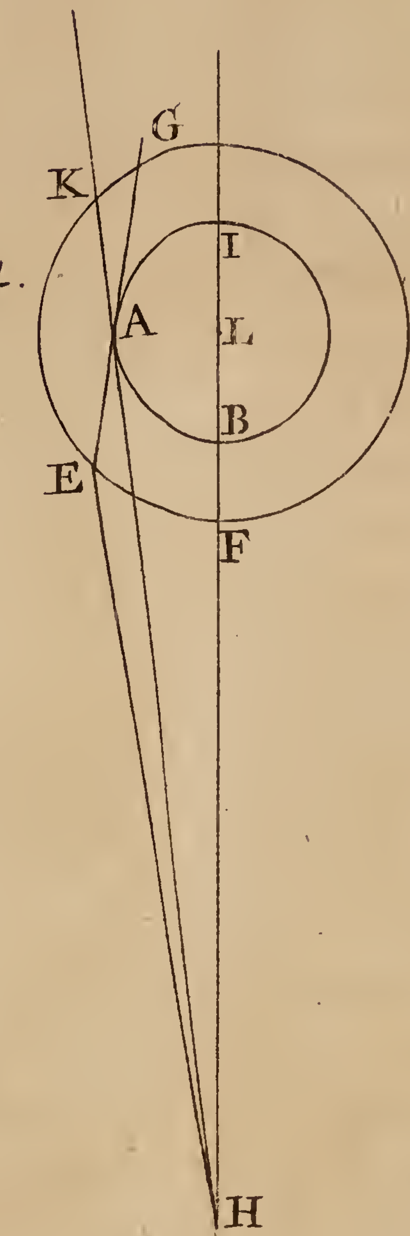


Fig. 75.

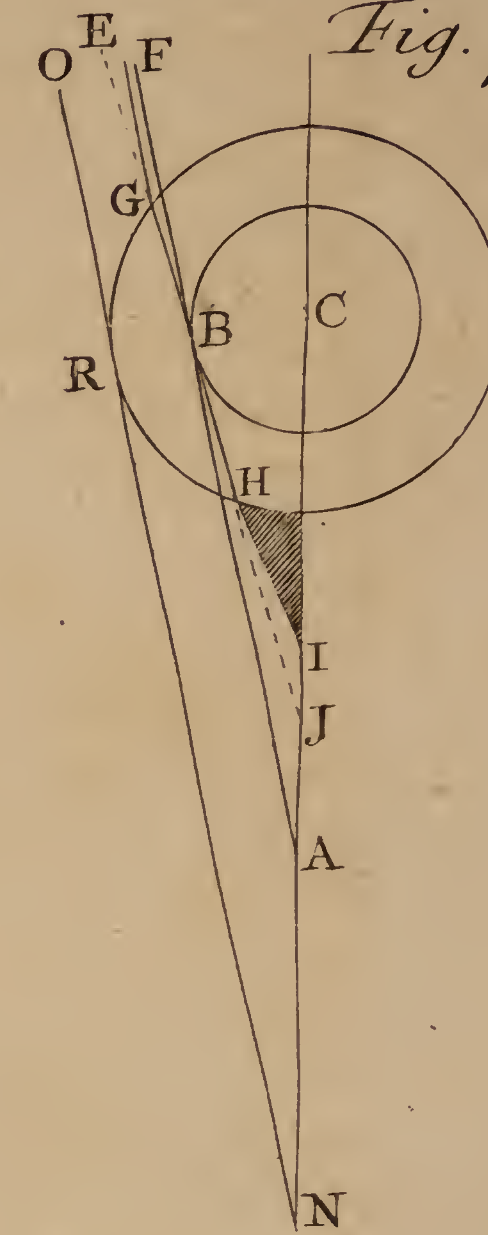


Fig. 76.

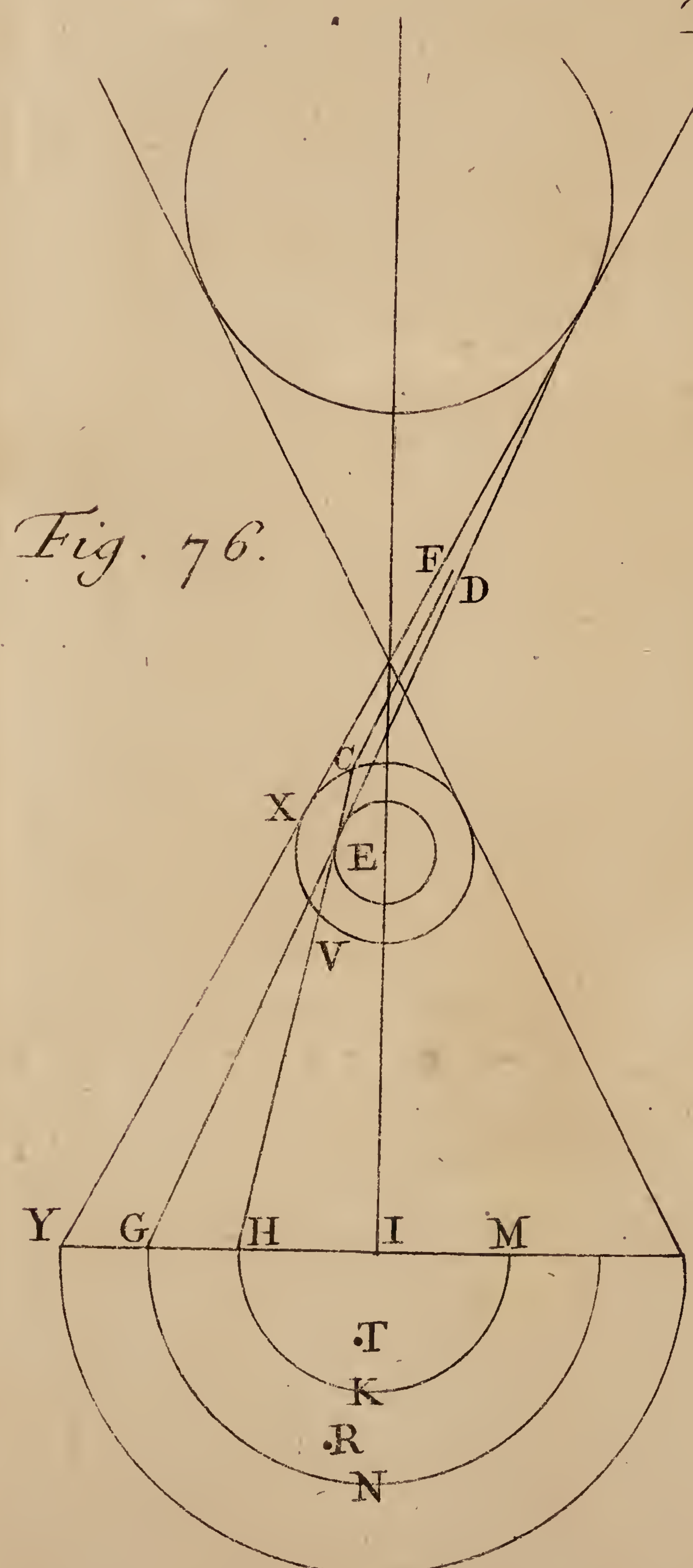
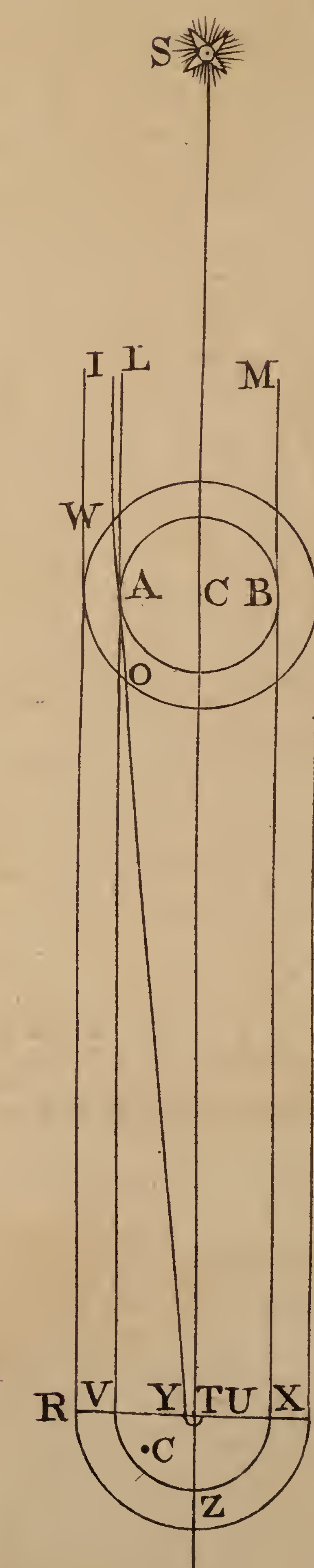


Fig. 77.



Feb. 19. 7^h $15'$ p. m. the Diſtance of ♂ from ο♓ was obſerved, namely ο♓ ♂ 1° $17'$ $30''$. Mars was diſtant from the Star toward the North.

XXI. 1.

Eclipſes of the Satellites of Jupiter *obſerved by* Euſtachius Manfredi. No. 429. p. 117. July, &c. 1733.

True Time. h	′	″	*April* 2, 1732.
10	56	3	Emerſion of the ſecond Satellite of Jupiter out of the Shadow. Sky clear. Teleſcope of 22 Feet.
13	2	36	Emerſion of the fourth Satellite, Sky clear, Teleſcope 22 Feet.
7	31	40	*April* 3. Emerſion of the inner Satellite from the Shadow, Sky clear, Teleſcope 22 Feet.
13	32	4	*April* 9. Emerſion of the ſecond Satellite, Sky clear, Teleſcope 22 Feet, a little doubtful.
9	44	41	*May* 3. Emerſion of the inner Satellite, Sky clear, Teleſcope 22 Feet.
			May 4. Emerſion of the ſecond Satellite, Sky cloudy, Wind.
10	35	32	Teleſcope 14 Feet.
10	35	41	Teleſcope 11 Feet.
			May 26. Emerſion of the inner Satellite, Sky clear.
9	58	4	Teleſcope 22 Feet.
9	58	21	Teleſcope 11 Feet.
9	43	47	*June* 2. Emerſion of the third Satellite, Sky cloudy, Teleſcope 22 Feet.
11	7	24	*June* 9. Immerſion of the third Satellite into the Shadow, Sky clear, Teleſcope 22 Feet.
10	8	25	*June* 18. Emerſion of the inner Satellite, Sky clear, Teleſcope 22 Feet.
7	36	5	*July* 27. Emerſion of the inner Satellite, Sky clear, Teleſcope 11 Feet, doubtful.
			Jan. 17, 1733. Immerſion of the third Satellite, Sky clear.
14	8	45	Teleſcope 22 Feet.
14	8	33	Teleſcope 14 Feet.
16	13	29	Emerſion of the third Satellite, Sky clear, Teleſcope 22 Feet.
			March 12. Immerſion of the inner Satellite, Sky clear.
13	23	34	Teleſcope 22 Feet.
13	23	22	Teleſcope 11 Feet.

— *Observed by* Geo. Lynn, *Esq; at* Southwick, *near* Oundle *in* Northamptonshire. No. 440 p. 196. Jan.&c.1736.

2. The Telescope I made use of is the same as formerly, having a 13 Foot Object-Glass, with an Aperture of $2\frac{4}{10}$ Inches, and an Eye-Glass of $2\frac{1}{2}$ Inches. By apparent Time, at *Southwick*, near *Oundle* in *Northamptonshire*, Longitude West from *London*, 00° 30′, as follows:

Year		Month.	D.	h	′	″
1730.	The 2d Satellite began to emerge —	*April*	29	10	19	20
	It seemed in a Minute after to be as bright as the 4th Satellite — —				20	20
	And at full Brightness about — —				21	20
17$\frac{29}{30}$.	The 4th Satellite emerged — — —	*Jan.*	25	9	45 nearly	
1730.	The 2d began to immerge — — —	*Nov.*	28	13	17	46
	And was quite out of Sight at — —				19	46
1731.	The 2d began to emerge — — —	*March*	29	11	33	8
	And seemed at full Brightness about— —				36	30
	The 1st Satellite began to emerge —	*April*	18	11	45	10
	And was at full Brightness about —				46	10
	Again the first began to emerge — —	*May*	4	10	4	30
	Was at full Brightness about — —				5	45
	And passed by the 3d Satellite at — —			11	49	30
17$\frac{30}{31}$.	The 1st Satellite began to immerge —	*Jan.*	7	12	2	55
	And was quite out of Sight about —				4	25
17$\frac{31}{32}$.	The 3d Satellite began to immerge —	*Jan.*	28	14	16	0
	Was but equal in Light to the 2d at —				19	0
	Quite gone at — — — — —				24	0
1732.	The 2d Satellite began to emerge — —	*April*	30	12	23	57
	The 1st Satellite began to emerge —	*May*	6	12	48	44
17$\frac{32}{33}$.	The 3d Satellite quite disappeared —	*Febr.*	18	13	5	30
1733.	Again the 3d Satellite disappeared —	*April*	2	13	3	30
	But it began to fail of it's Light about 5 or 6 Minutes before.					
1735.	The 2d Satellite immerged — — —	*May*	28	10	45	0
	The 3d began to emerge — — —	*August*	3	9	10	30
	And was 4 or 5 Minutes before it came to it's full Brightness.					

— *Observed at* Petersburg, *by* M. Jos. Nic. De l'Isle. *F. R. S.* No. 441. p. 225. Ap.&c.1736.

3. It was a great Pleasure to me to see that Mr *James Hodgson* has been at the Pains to calculate the Eclipses of the four Satellites of *Jupiter*, which were to happen in 1732. It was to be wished he would continue to do so for the Years following; but I would advise him, to do it a long while beforehand, that People in Foreign Countries might have Time to be informed of it. He says, he has made use of Tables of the Satellites, which have not been corrected these 50 Years*. Probably he means the Tables of M. *Cassini*, published at the Royal Printing-House at *Paris*, in 1693, at the End of the Observations of the Gentlemen of the Academy made in several

* *He means Tables of the late Mr* Flamstead.

ſeveral Voyages. However, the late M. *Caſſini*, has from Time to Time made divers Corrections to thoſe Tables, though they never were made publick. M. *Maraldi* has alſo much worked at it after the Death of M. *Caſſini*, and has communicated to me his Corrections, on which I have taken Pains to calculate new Tables; but having in the Year 1724, received of Dr *Halley* a Copy of his Aſtronomical Tables, among which, are thoſe of the four Satellites of *Jupiter* by Mr *Bradley*, I judged there could not be any better, till ſome Method ſhall be found and explained geometrically to deduce from the Laws of Gravity, the Effect of the mutual Attraction of theſe Satellites on one another, and with relation to *Jupiter*: But as I could not hope this could be done ſo ſoon, I took the Pains again to calculate new Tables upon thoſe of Mr *Bradley*, by reducing the Tables of the four Satellites into the ſame Form with thoſe Mr *Pound* has made of the firſt Satellite only. Theſe Tables being thus made eaſy, I have uſed them hitherto for comparing Obſervations; and my Brother has taken the Pains, ſince the drawing up thoſe Tables, in the ſaid Manner, to calculate a Year beforehand all the Eclipſes of the four Satellites. I commonly ſent thoſe Calculations to my Correſpondents, to prepare them for Obſervations; and ſome Years of thoſe Ephemerides have been publiſhed in the *little Gazette of Literature* of *Leipzig*, printed in *High Dutch*. My Brother lately prolonged theſe Calculations to the Month of *January*, 1737.

Herewith follow the laſt Obſervations on the Satellites of *Jupiter*, which were made at *Petersburg*, ſince thoſe inſerted in the third Volume of the Memoirs of the Academy of *Petersburg*, to the preſent Time.

N. St.	True Time h ′ ″	
1731. *Dec.* 6	17 3 5	Immerſion of the firſt Satellite difficultly obſerved with a reflecting Teleſcope of 5 Foot. The true Time was found only by Means of two Clocks.
1732. *Jan.* 4	13 30 56	Immerſion of the ſecond by the Reflecter, doubtful to a few Seconds. *Jupiter* not being well defined nor ſufficiently high. The true Time adjuſted by two Clocks.
9	18 33 7	Immerſion of the fourth by the Reflecter. The Sky not very ſerene, and the true Time adjuſted only by two Clocks.

The

N. St.			True Time h ′ ″	
1732.	*Jan.*	9	20 25 0	The other Satellites difappearing by the Day-light, the fourth was not yet come out of the Shadow. Telefcope the fame.
	Feb.	22	13 25 34	The firft Satellite, juft entering the Shadow, was yet vifible when a Mift covered *Jupiter*.
			13 26 34	*Jupiter* being uncovered, the firft Satellite did not now appear through the reflecting Telefcope. The true Time was adjufted only by two Clocks.
	March	8	8 22 20	Immerfion of the third by the reflecting Telefcope. The Wind was fomewhat troublefome, the true Time was adjufted by two Clocks.
	April	3	8 46 23	Emerfion of the firft by the reflecting Telefcope. Doubtful to a few Seconds, by reafon of the Nearnefs of the Satellite to *Jupiter*.
		13	7 20 30	Immediately after Sunfet, *Jupiter* becoming vifible to the Eye, the third Satellite appeared to be out of the Shadow, and entirely clear by the reflecting Telefcope.
		20	11 6 52	Emerfion of the third Satellite, by the reflecting Telefcope. The Sky ferene.
		27	15 13 0	The third Satellite had been come out of the Shadow perhaps for feveral Minutes; for the other Satellites did not appear better than this which was feen with the reflecting Telefcope through the Mift, *Jupiter* being low, and the *Crepufculum* ftrong.
	May	10	12 55 54	Emerfion of the firft by the reflecting Telefcope. Sky ferene. Obfervation certain.
		26	11 14 5	Emerfion of the firft by a Telefcope of 13 Foot. Cloudy.
	Dec.	24	18 4 30	Immerfion of the 1ft by a Telefcope of 13 Foot. A good Obfervation.

4. Full

— *Obferved by the Jefuit's at the College at* Pekin. No 468. p. 306. Jan. 1742-3.

4.	h	′	″			
1740. *Nov.* 4.	5	55	15	*a. m.*	Full Immerfion of the firft Satellite, Telefcope 13 Feet.	
8.	12	16	5	*p. m.*	Full Immerfion of the 2d Satellite.	
12.	14	16	52	*p. m.*	Full Immerfion of the 1ft Satellite.	
15.	14	49	24	*p. m.*	Full Immerfion of the 2d Sattellite.	
16.	9	55	0	*p. m.*	As the 3d Satellite was going to immerge into the Shadow, it difappeared in a Cloud, fo that the Immerfion neither of this nor of the 4th could be feen.	
19.	16	9	10	*p. m.*	Full Immerfion of the 1ft Satellite.	
21.	10	37	33	*p. m.*	Full Immerfion of the fame.	
22.	17	21	48	*p. m.*	Full Immerfion of the 2d Satellite.	
26.	18	1	25	*p. m.*	Full Immerfion of the 1ft Satellite.	
30.	17	47	40	*p. m.*	Full Immerfion of the 3d Satellite.	
Dec. 3.	9	10	57	*p. m.*	Full Immerfion of the 2d Satellite.	
10.	11	42	32	*p. m.*	Full Immerfion of the 2d Satellite, Telefcope 10 Feet.	
14.	10	38	20	*p. m.*	Full Immerfion of the 1ft Satellite, Telefcope 10 Feet.	
17.	14	15	20	*p. m.*	Full Immerfion of the 2d Satellite, Telefcope 10 Feet.	
19.	18	3	0	*p. m.*	Immerfion of the 1ft Satellite, Telefcope 10 Feet.	
1741. *Jan.* 1.	5	30	6	*p. m.*	Emerfion of the 1ft Satellite, Telefcope 10 Feet.	
6.	12	52	25	*p. m.*	Emerfion of the 1ft Satellite, Telefcope 18 Feet.	
8.	7	20	30	*p. m.*	Emerfion of the fame, fame Telefcope, doubtful.	
11.	13	51	22	*p. m.*	Emerfion of the 2d Satellite. Telefcope 10 Feet.	
14.	2	42	40	*a. m.*	Emerfion of the 1ft Satellite. Telefcope 18 Feet.	
15.	9	12	36	*p. m.*	Emerfion of the fame. Telefcope 13 Feet.	
18.	16	26	15	*p. m.*	Emerfion of the 2d, Telefcope 10 Feet.	
22.	5	41	57	*p. m.*	Emerfion of the 2d, Telefcope 18 Feet.	
	11	3	19	*p. m.*	Emerfion of the 1ft, Telefcope 18 Feet.	
24.	5	33	24	*p. m.*	Emerfion of the 1ft, Telefcope 10 Feet.	
29.	8	18	16	*p. m.*	Emerfion of the 2d, Telefcope 13 Feet.	
	12	57	10	*p. m.*	Emerfion of the 1ft,	
Feb. 3.	8	18	0	*p. m.*	Emerfion of the 3d,	Telefcope 8 Feet.
5.	10	53	20	*p. m.*	Emerfion of the 2d,	
6.	2	53	0	*p. m.*	Emerfion of the 1ft,	

1741.	h	′	″		
Feb. 7.	9	20	35	*p. m.*	Emerſion of the 1ſt, Teleſcope 8 Feet.
8.	10	43	0	*p. m.*	Full Immerſion of the 4th, Teleſcope 13 Feet.
	14	6	30	*p. m.*	The 4th began to immerge. Same Teleſcope.
10.	9	16	30	*p. m.*	Full Immerſion of the 3d. Same Teleſcope.
12.	13	32	0	*p. m.*	The 2d immerged, Teleſcope 8 Feet.
14.	11	14	15	*p. m.*	Emerſion of the 1ſt, Teleſcope 18 Feet.
16.	5	43	45	*p. m.*	Emerſion of the ſame, ſame Teleſcope.
23.	7	39	29	*p. m.*	Firſt emerged, Teleſcope 18 Feet.
25.	8	26	30	*p. m.*	Emerſion of the 4th, Teleſcope 13 Feet.
Mar. 2.	0	36	11	*p. m.*	Emerſion of the 1ſt, Teleſcope 18 Feet.
11.	6	2	45	*p. m.*	Emerſion of the ſame, ſame Teleſcope.
Apr. 3.	6	26	35	*p. m.*	Emerſion of the ſame, ſame Teleſcope.
	8	14	0		Emerſion of the 2d, Teleſcope 8 Feet.
10.	8	20	37	*p. m.*	Firſt emerged, Teleſcope 13 Feet.
May 3.	8	40	6	*p. m.*	Emerſion of the 1ſt, ſame Teleſcope.
	9	36	0		Emerſion of the 4th, ſame Teleſcope.
Sept. 8.	4	41	48	*a. m.*	Immerſion of the 1ſt, ſame Teleſcope.
Oct. 1.	5	0	8	*a. m.*	Immerſion of the 1ſt, Teleſcope 13 Feet.
15.	17	8	49	*p. m.*	Immerſion of the 2d, Sky a little cloudy, Teleſcope 13 Feet.

An Occultation of Jupiter *and his* Satellites *by the* Moon, October 27, 1740, *in the Morning; obſerved at Mr* George Graham's, *F.R.S. Houſe in* Fleet-Street, London, *by Dr* Bevis, *and Mr* James Short, *F.R.S.* No. 459. p. 647.

XXII.

Times by the Clock, *October* 26. Clock above Stairs.			Apparent Times. *October* 27.			
h	′	″	h	′	″	
23	46	38	0	0	0	The Sun's Centre paſſed the Meridian in the Tranſitory.
Clock below.						
14	49	4	15	2	25	The Moon's illuminate Limb preceded *Jupiter* in right Aſcenſion 1′ 38″ in Time.
14	52	32	15	5	53	The ſame Limb preceded *Jupiter* 1′ 31″. Theſe were taken with a reflecting Teleſcope, 9 Inches Focus, fitted with Wires at half right Angles, and which magnified 30 times.
Clock above.						
15	26	1	15	39	20	*Sirius* paſſed the Meridian.
15	37	43	15	51	2	The Moon's Centre paſſed the Meridian.
15	39	9	15	52	28	*Jupiter*'s Centre paſſed the Meridian.

Clock

Times by the Clock, *October* 26. Clock below Stairs.			Apparent Times. *October* 27.			
h	′	″	h	′	″	
15	41	15	15	54	36	*Jupiter*'s third Satellite eclipſed by the Moon.
15	47	10	16	0	31	*Jupiter*'s ſecond Satellite eclipſed by the Moon.
15	55	4	16	8	25	*Jupiter*'s preceding Limb immerged.
15	57	20	16	10	41	*Jupiter*'s ſubſequent Limb immerged.
16	0	54	16	14	15	*Jupiter*'s firſt Satellite eclipſed by the Moon.
						Theſe Immerſions were taken with a Reflecting Teleſcope, of 16.5 Inches Focus, that magnified 120 times.
Clock above.						
16	17	49	16	31	8	*Procyon* paſſed the Meridian.
Oct. 27.			*Oct.* 28.			
23	46	42	0	0	0	The Sun's Centre paſſed the Meridian.

N. B. The Clock in the lower Room was all along 2″ ſlower than the Clock in the upper Room.

None of the Emerſions could be ſeen for Clouds. Whilſt *Jupiter* was immerging, the Sky was perfectly ſerene; and, at his neareſt Approach to the Moon, he did not appear to alter his Figure in the leaſt, nor to be tinged with any priſmatic Colours; neither did he (as is ſaid to have been ſometimes obſerved through Refracting Teleſcopes) ſeem to enter at all upon the Moon's Body.

That Part on the Moon's Limb where *Jupiter* entered, was a Hollow; and though ſome are of Opinion, that the Circumference of the Moon, as it is bounded to our Eye, is a perfectly ſmooth Circle, and that no Hills or Hollows appear there, as on the Surface of the Moon; yet if it be looked at in a clear Night with a good Teleſcope, that magnifies about 100 times, or even leſs, it will be ſeen rugged and uneven all round.

Notwithſtanding *Jupiter*'s Light ſeems to be more vivid than that of the Moon, when he is ſeen at a good Diſtance from her, and far more ſo when the Moon is away; yet the contrary is plainly diſcerned when they are near one another: And in this Obſervation, whilſt *Jupiter* was immerging behind the Moon, his Diſk appeared much dimmer, and of a more faint and duſky Complexion, than the Diſk of the Moon.

An Occultation of Mars *by the* Moon, Oct.7. 1736, *observed by Mr* Geo. Graham, *F. R. S. in* Fleet-street, London, No. 446. p. 100. July, &c. 1737.

XXIII. 1. The first Contact could not be seen for Clouds.

Apparent Time.

	h	'	''	
At	14	24	44	*Mars* appeared about half covered, but a distinct View could not be had for flying Clouds.
	14	25	21	*Mars* totally covered, the last Ray of Light being then lost.
	15	11	22	The *Moon* appeared, but *Mars* was not seen, no Part being yet emerged.
	15	15	11	I judged it was quite emerged, but Clouds prevented the *Moon*'s Limb from being distinctly seen.

The Observation was made with a Refracting Telescope of 12 Feet.

— Observed in Covent-Garden, *by* J. Bevis, *M. D.* Ibid. p. 101.

2. Before the Eclipse, I took several Differences of Right Ascension and Declination between ♂ and *μ. Piscium*, for ascertaining the true Place of *Mars*: As also several Differences of right Ascension and Declination between the *Moon* and *Mars*, before and after the Eclipses which I shall give another Time.

Apparent Time.

h	'	''	p. m.
14	24	10	I was surprized to see *Mars* continue quite round, though hardly, to Appearance, disjoined from the scabrous Edge of the *Moon*; but that Instant I thought it began to lose it's Figure. Clouds.
14	25	26	The *Moon* shone out bright again, but *Mars* was entirely vanished.
15	14	46	The *Moon* being just clear of a Cloud, I saw *Mars* partly emerged.
15	14	49	He seemed just half out; then Clouds came on again, so that I saw not the final Contact.

The *Moon*'s Diameter was 21,157 Parts of the Micrometer and it's illuminated Part passed over the horary Thread in 2 Minutes, 3 Seconds.

I am certain of the Time to 2 or 3 Seconds.

Occultation of Mars *by the* Moon, *observed by the Jesuits at* Pekin, No. 468. p. 306. Jan. 1742-3.

3. *Nov.* 9, 1740. 11^h 2' 15'' *Mars* emerged from the *Moon*, in a right Line drawn through Menelaus and Kepler; the Immersion could not be seen because of Clouds in the eastern Horizon.

XXIV. § 1.

XXIV. §. 1. *October* 10, 11, and 12, when ♂ passed near μ ♓, a Star of the 5th Magnitude, I observed the following Distances of the Centre of Mars from that Star.

Observations on Mars in the Autumn of 1736, at Berlin, by Christ. Kirch, *Astronomer of the R. Society there.* No. 459. p. 573. Jan. &c. 1741.

Conjunction of Mars & μ ♓.

St. N.	True Time.			Parts of the Micromet.	Value of Parts of the Mic.	
	h ′				′	″
Oct. 10	9 41 *Vesp.*	♂ μ ♓	Telesc. 7 F.	48½	19	24
	9 46		Telesc. 9 F.	65	19	21
Oct. 11	10 1	♂ μ ♓	Telesc. 9 F.	22½	6	42
	10 4		Telesc. 7 F.	16	6	24
	10 9		Telesc. 7 F.	16½	6	36
	10 12		Telesc. 9 F.	22	6	33
Oct. 12	8 55 *Vesp.*	♂ μ ♓	Telesc. 9 F.	71½	21	18
	8 59		Telesc. 7 F.	53	21	12
	9 5		Telesc. 7 F.	53½	21	24

§ 2. In order to obtain from these Distances the Time of the Conjunction of Mars with the Star μ ♓, I chose the three following Distances.

				′	″
		h ′			
1.	*Oct.* 10.	9 43 *Vesp.*	Distance of the Centre of ♂ from μ ♓ — — — — — —	19	22
2.	*Oct.* 11.	10 6 *Vesp.*	— — — — — — —	6	34
3.	*Oct.* 12.	9 0 *Vesp.*	— — — — — — —	21	18

I supposed from the Ephemerides, the diurnal Motion of Mars in Longitude 19′ 30″, in Latitude 3′ 40″: Therefore the diurnal Motion of Mars in his proper Orbit, is 19′ 51″, and the Angle of the Orbit of Mars and the Ecliptick (or rather with the Parallel of the Ecliptick) is 10° 39′.

§ 3. In the oblique-angled Triangle $a\mu b$, three Sides being given namely, Fig. 78.

	′	″
ab, the Motion of Mars, which answers to $24^h 23'$ (the Time between Obs. 1 and 2) — — — — — —	20	10
$a\mu$, the Distance first observed — — — — — —	19	22
$b\mu$, the Distance secondly observed — — — — —	6	34
From μ I drew a Perpendicular to the apparent Orbit of Mars $\mu\chi$, and in the rectangular Triangle $b\chi\mu$ sought the Particle of the Orbit of Mars $b\chi$, and found it to be —	1	51
and the least Distance of ♂ and μ, $\chi\mu$, which I found to be —	6	18

The

h ′

The Particles of the Orbit of Mars $\chi\,b$ anfwer to — 2 14
Which being fubtracted from the fecond Time of Obfervation, *Oct.* 11. — — — — — — — } 10 6 *Vefp.*
Leave the true Time ☌ ♂ μ ♓ in the Orbit, *Oct.* 11. — 7 52 *Vefp.*
§. 4. In the oblique angled Triangle $b\,\mu\,c$ the Motion of Mars between Obf. 2, and 3, $b\,c$, is — — — — — } 18′ 55″
The Diftance of ♂ from μ ♓ by the Obf. 2. $\mathfrak{b}\,\mu$ — — — 6 34
The Diftance of ♂ from μ ♓ by Obf. 3. $c\,\mu$ — — — 21 18

Thefe three Sides being given, I fought the Angle c, and found it to be 17° 35′. Then I drew a Perpendicular from μ to the Orbit of Mars $\mu\,\chi$, and in the rectangular Triangle $c\,\chi\,\mu$, the Hypothenufe $c\,\mu$ being given, I fought the Sides $\mu\,\chi$, and $c\,\chi$, and found $\mu\,\chi$, the leaft Diftance 6′ 26″ and $c\,\chi$ — — — — — 20′ 18″
From which the Side $b\,c$ being fubtracted — — — — 18 55
Leaves $b\,\chi$ — — — — — — — — — — 1 23
To which anfwers in Time — — — — — 1h 40′
Which being fubtracted from the Time of Obf. 2. *Oct.* 11. 10 6 *Vefp.*
Gives the true Time of the leaft Diftance, or of the Conjunction of ♂ and μ ♓ in the Orbit, *Oct.* 11. } 8 26 *Vefp.*

§. 5. The Deductions in the two preceding Paragraphs, as ufual, differ but little. If I had taken the diurnal Motion of Mars about ¼ of a Minute lefs, the Difference would have been fmaller. In the mean Time, if I choofe a mean between the 2 Deductions, I can err but very little from the Truth; and thus I gather the true Time of ☌ ♂ μ ♓ in the Orbit of Mars, *Oct.* 11. 8h 9′ the leaft Diftance of ♂ from μ ♓ 6′ 22″ North.

§ 6. Tho' this might have been fufficient, yet I fet about a new Calculation, fuppofing the diurnal Motion of ♂ in Longitude — — — — — — — — — — } ′ ″ 19 15
In Latitude — — — — — — — — — — 3 40
Therefore the diurnal Motion of ♂ in the Orbit was — — 19 36

And the Angle of the Orbit of Mars with the Parallel of the Ecliptick 10° 47′ the Spaces of Time between Obf. 1, and 2, and between 2 and 3, this diurnal Motion of Mars in the Orbit 19′ 36″ being given, make $a\,b$ 19′ 55″, and $b\,c$ 18′ 42″; the Diftances, $a\,\mu$, $b\,\mu$ and $c\,\mu$ remain the fame as in the former Calculations. Thefe being granted, I found in the firft Place by the Triangle $a\,b\,\mu$, $\mu\,\chi$ 6′ 22″, and $b\,\chi$ 1′ 37½″.

h ′

To which anfwers — — — — — — — — 1 59
Which being fubtracted from *Oct.* 11. — — — 10 6 *Vefp.*
Leaves the Time of the leaft Diftance, *Oct.* 11. — — 8 7 *Vefp.*
Then by the Triangle $b\,c\,\mu$, I found $\mu\,\chi$ — — — — 6′ 21″

And

	h	′
And $b\chi$ 1′ 38″, which anfwer in Time to — — —	2	0
Which being fubtracted from the Time of Obf. 2. *Oct.* 11. — — — — — — — — —	10	6 *Vefp.*
Leave the Time of the leaft Diftance, *Oct.* 11. — —	8	6 *Vefp.*

Thus thefe Calculations agree very well together, and with the mean of the former Calculations.

§. 7. If from μ a Right Line $\mu\, d$ be drawn, which with the Line $\chi\, \mu$, perpendicular to the Orbit of Mars, makes an Angle at μ equal to the Angle of the Orbit of Mars with the parallel of the Ecliptick, $d\, \mu$ will be perpendicular to the Ecliptick. I found this Angle at firft to be 10° 39′ (§. 2.); and then the Diurnal Motion of Mars being corrected, I found it to be 10° 47′ (§. 6.). Now in the Rectangular Triangle $d\, \chi\, \mu$, befides the Angles, the Side $\chi\, \mu$ is known to be 6′ 22″, and the other Sides are fought. Affuming therefore the Angle $\chi\, \mu\, d$ 10° 39′, the Side $\chi\, d$ is found to be 1′ 12″. But if I make ufe of the more correct Angle 10° 47′, the Side $\chi\, d$ will be 1′ 13″.

		h	′	″
To which is anfwerable in Time — — — — —		1	29	0
Which being added to the time of the leaft Diftance *Oct.* 11. — — — — — — — — —		8	7	0
Gives the true time of ☌ ♂ and μ ♓ in the Ecliptick *Oct.* 11. — — — — — — — —		9	36	0
$d\, \mu$, or the difference of the Latitude of Mars, from the Latitude of the Star in ☌ in the Ecliptick is found — — — — — — — — — —		0	6	29

		°	′	″
Which being fubtracted from the Latitude of the Star — — — — — — — — —		3	4	25 *S.*
Leaves the Latitude of Mars — — — — —		2	57	56 *S.*
The Longitude of Mars is equal to the Longitude of the Star according to the accurate *Britannick Catalogue* — — — — — — — —	♈	19	25	40

		h	′	″
§. 8. At the Time of the Conjunction of Mars and μ ♓ in the Ecliptick, at *Berlin*, true time *October* 11. — — — — — — — — — —		9	36	0
And at *Bologna*, mean Time *Oct.* 11 — — — —		9	14	0
By *Manfredi*'s Ephemerides the Longitude of Mars is found to be — — — — — — — —	♈	19°	14	40
Which falls fhort of the Obfervation — — — —			11	0
Ghifler's Ephemerides make the Longitude of ♂ —	♈	19	4	0
Almoft 22′ fhort of the Obfervation, and the Ephem. of *Defplaces* make it — — — — — —	♈	19	25	0

Agreeable enough to the Obfervation.

Manfredi's

	°	′	″
Manfredi's Ephemerides make the S. Latitude of ♂ that is, about 1′ less than the Latitude observed;	2	57	0
According to *Ghisler*'s it is — — — — — —	2	57	30
And according to those of *Desplaces* — — — —	2	59	30

The Place of Mars *in opposition to the* Sun.

	°	′	″
§. 1. At the Time of the Conjunction of Mars and μ ♓ in the Ecliptick, the Place of the Sun is found by *Manfredi*'s Tables to be — — — — —	6 ♎ 18	46	21
At which time the Longitude of Mars was to —	0 ♈ 19	25	40

And therefore ♂ was almost in opposition to the Sun, and only 39′ 19″ from the opposite Place to the Sun.

The diurnal Motion of the Sun was — — — —	0	59	34
And the diurnal Motion of ♂ retrograde in the Eclipt. —	0	19	15
The Sum gives the diurnal Motion of ☉ from ♂ — —	1	18	49

	h	′	″
§. 2. As 1° 18′ 49″ the diurnal Motion of ☉ from ♂ is to 24 hours, so is 39′ 19″, the Distance of ♂ from the opposite Place to ☉ to — — — — — —	11	58	0
Which being added to the true Time of ☌ ♂ μ ♓ in the Ecliptick *Oct.* 11. — — — — — —	9	36	0
Makes the Time of the Opposition of Mars and the Sun, at *Berlin*, *Oct.* 11. true time — — — —	21	34	0
Subtract the Equation — — — — — — —	0	13½	
There will remain mean Time at *Berlin*, *Oct.* 11. —	21	20½	
For the Difference of Meridians, between *Bologna* and *Berlin*, subtract — — — — — — —	0	8½	
Remains mean Time at *Bologna*, *Oct.* 11. — — —	21	12	0

	°	′	″
§. 3. As 24^h to 19′ 15″ the diurnal Motion of Mars in Longitude, so 11^h 58′, the time between ☌ ♂ and μ ♓ in the Ecliptick, and the Opposition of ☉ and ♂, to	0	9	36
Which being subtracted from Longitude ♂ in ☌ ♂ and μ ♓ — — — — — — — —	0 ♈ 19	25	40
Leaves Longitude of ♂ in ☍ ☉ — — — —	0 ♈ 19	16	4
The Place of ☉ by *Manfredi*'s Tables *Oct.* 11. 21^h 12′ mean Time at *Bologna* is found — —	6 ♎ 19	16	3

A Difference of only 1″ (besides the Semi-circle) from the Place of Mars, which may safely be neglected.

	°	′	″
§. 4. As 24^h to 3′ 40″, the diurnal Motion of Mars in Latitude, so 11^h 58′ to — — — —	0	1	50
Which being subtracted from Latitude ♂ in ☍ ♂ and μ ♓ in the Ecliptick — — — — — —	2	57	56 S.
Leaves Latitude ♂ in ☍ ☉ — — — — — —	2	56	6 S.

Observations on Mars *about*

Mars was among the Stars *e*, ε and ζ of *Pisces*, and other smaller Stars; from which I often measured the distances of Mars, with 3 different

different Telescopes, of 7, 9 and 2 feet, and once with a Telescope of 18 feet. By the longer Telescopes more accurate Distances may be taken; but because they do not comprehend any great Space, I could measure only the smaller Distances by them. Large Distances might indeed be observed by the Telescope of 2 feet, but sometimes a Doubt of 1 or 2 Minutes may creep in, especially if the Distances are too large for the Capacity of the Telescopes Such Errors are most seen when the Situation of the Stars is drawn upon Paper, and the Distances of a Planet from different Stars, do not intersect each other in one Point. I have taken out the Stars, from which I measured Mars, from the *Britannick Catalogue*, and by the Distances of Mars from these Stars, I have traced out the Place of the Planet by means of a Circle. I shall first exhibit the Distances taken, and then the Places of Mars found thereby. Where it is to be observed, that I have made use of a Delineation, in which the Magnitudes of Degrees and Distances of the Stars were double of those in the Scheme.

his second Station November 1736.

Fig. 79.

Styl. nov.	True time Vesp. h	'			Parts of the Micrometer	Value of parts of the Microm. °	'	"
Oct. 27.	8.	58.	♂ e ♓	Telef. 7 feet	121.	0.	48.	24.
Oct. 29.	8.	30.	♂ e ♓	Telef. 7 feet	6.		24.	48.
	8.	38.		Telef. 9 feet	83.		24.	43.
Nov. 1.	11.	6.	♂ e ♓	Telef. 9 feet	38.		11.	18.
	11.	10.		Telef. 7 feet	28.		11.	12.
Nov. 5.	7.	22.	♂ a.	Telef. 7 feet	34 ½.		13.	48.
	7.	26.	♂ e ♓	. . .	100 ½.		40.	12.
	8.	14.	♂ c.	. . .	105.		42.	0.
	8.	21.	♂ a.	Telef. 9 feet	44.		13.	6.
Nov. 6.	7.	28.	♂ a.	Telef. 7 feet	17.		6.	48.
	7.	34.	♂ e ♓	. . .	116.		46.	24.
	7.	40.	♂ c.	. . .	110.		44.	0.
			or		110 ½.		44.	12.
	7.	44.	♂ a.	Telef. 9 feet	23 ½.		6.	59.
Nov. 7.	6.	4.	♂ a.	Telef. 18 feet	16.		2.	17.
	7.	47.	♂ a diffic.	Telef. 7 feet	5 ½.		2.	12.
	7.	50.	♂ e ♓	. . .	129.		51.	36.
	7.	53.	♂ c.	. . .	118.		47.	12.
Nov. 12.	9.	19.	♂ a.	Telef. 7 feet	52.	0.	20.	48.
	9.	27.	♂ e ♓	. . .	172.	1.	8.	48.
	9.	38.	♂ c.	. . .	165.	1.	6.	0.

Styl. nov.	True time Vefp. h	'			Parts of the Micrometer	Value of parts of the Microm. °	'	"
Nov. 13.	7.	32.	♂ a.	Telef. 9 feet	77.	0.	22.	56.
	7.	36.	♂ a.	Telef. 7 feet	58.	0.	23.	12.
	7.	40.	♂ e ♓.	. . .	175.	1.	10.	0.
	7.	44.	♂ c.	. . .	171.	1.	8.	24.
Nov. 15.	7.	2.	♂ a.	Telef. 7 feet	72.	0.	28.	48.
	7.	9.	♂ e ♓.	. . .	179.	1.	11.	36.
	7.	13.	♂ c.	. . .	186 ½.	1.	14.	36.
	7.	18.	♂ a.	Telef. 9 feet	96.	0.	28.	35.
Nov. 26.	6.	11.	♂ e ♓.	Telef. 2 feet	91.	1.	22.	6.
			♂ ζ ♓.	. . .	106.	1.	35.	38.
			♂ ε ♓.	. . .	94.	1.	24.	48.
			♂ c.	. . .	143.	2.	9.	2.
			♂ a.	. . ♓	113.	1.	41.	57.
			♂ e ♓.	. . .	92.	1.	23.	0.
	6.	32.	♂ a.	. . .	103.	1.	32.	55.
Nov. 28.	6.	43.	♂ e ♓.	Telef. 2 feet	104.	1.	33.	50.
	6.	46.	♂ ζ ♓.	. . .	82.	1.	13.	59.
	9.	34.	♂ ε ♓.	. . .	103.	1.	32.	55.
	9.	37.	♂ e ♓.	. . .	105.	1.	34.	44.
	9.	41.	♂ ζ ♓.	. . .	82.	1.	13.	59.
			better		81.	1.	13.	5.
Dec. 3.	9.	41.	♂ e ♓.	Telef. 2 feet	160.	2.	24.	23.
			♂ ε ♓.	. . .	157.	2.	21.	40.
	9.	52.	♂ ζ ♓.	Telef. 7 feet	56.	0.	22.	24.
	10.	1.	♂ ζ ♓.	Telef. 9 feet	75. ½		22.	29.
			or		76.		22.	38.
Dec. 6.	5.	33.	♂ ε ♓.	Telef. 2 feet	201.	3.	1.	22.
	5.	39.	♂ e ♓.	. . .	204.	3.	4.	4.
	5.	44.	♂ ζ ♓.	. . .	50.	0.	45.	8.
			♂ ζ ♓.	Telef. 7 feet	113 ½.	0.	45.	24.
	5.	57.	♂ ζ ♓.	Telef. 9 feet	153.	0.	45.	34.

Theſe Diſtances are always to be underſtood from the Centre of Mars, eſpecially by the longer Teleſcopes.

Now follow the Places of Mars deduced from the Diſtances enumerated, and his Places taken from the different Ephemerides, to ſhew the Agreement or Diſagreement between the Calculations and the Obſervation.

Styl. nov.	True Time Vesp. h '		Longitude of Mars. ° ' ''	Latitude of Mars. ° ' ''
Nov. 5.	8. 18.	Observation.	♈ 13. 37. 0.	1. 17. 0. S.
		Manfredi.	13. 26. —	1. 16. 30.
		Ghisler.	13. 42. —	1. 20. 30.
		Desplaces.	13. 35. 30.	1. 17. 30.
Nov. 6.	7. 26.	Observation.	♈ 13. 32. 0.	1. 13. 15. S.
		Manfredi.	13. 22. —	1. 13. —
		Ghisler.	13. 37. 30.	1. 16. —
		Desplaces.	13. 31. —	1. 14. —
Nov. 7.	7. 50.	Observation.	♈ 13. 27. 40.	1. 9. 30. S
		Manfredi.	13. 17. 40.	1. 9. 20.
		Ghisler.	13. 32. 40.	1. 12. —
		Desplaces.	13. 27. —	1. 10. —
Nov. 12.	9. 28.	Observation.	♈ 13. 20. 0.	0. 50. 40. S
		Manfredi.	13. 8. —	0. 51. —
		Ghisler.	13. 19. 30.	0. 53. —
		Desplaces.	13. 18. —	0. 51. 30.
Nov. 13.	7. 38.	Observation.	♈ 13. 20. 30.	0. 48. 0. S.
		Manfredi.	13. 9. 15.	0. 48. —
		Ghisler.	13. 19. —	0. 49. 30.
		Desplaces.	13. 19. —	0. 48. —
Nov. 15.	7. 12.	Observation.	♈ 13. 23. 30.	0. 42. 0. S.
		Manfredi.	13. 13. —	0. 41. —
		Ghisler.	13. 21. 30.	0. 43. —
		Desplaces.	13. 21. 30.	0. 41. —
Nov. 26.	6. 20.	Observation.	♈ 14. 35. 0.	0. 9. 0. S.
		Manfredi.	14. 26. —	0. 9. —
		Ghisler.	14. 37. —	0. 12. —
		Desplaces.	14. 34. —	0. 9. 30.
Nov. 28.	9. 35.	Observation.	♈ 14. 57. 0.	0. 3. 30. S.
		Manfredi.	14. 49. 30.	0. 4. —
		Ghisler.	14. 59. —	0. 7. —
		Desplaces.	14. 57. 30.	0. 5. —
Dec. 3.	9. 48.	Observation.	♈ 16. 1. 0.	0. 6. 30. N.
		Manfredi.	15. 57. —	0. 7. 40.
		Ghisler.	16. 3. —	0. 5. —
		Desplaces.	16. 2. —	0. 7. —
Dec. 6.	5. 46.	Observation.	♈ 16. 46. —	0. 16. — N.
		Manfredi.	16. 40. 30.	3. 13. 30.
		Ghisler.	16. 47. 30.	0. 10. 30.
		Desplaces.	16. 50. 33.	0. 13. 30.

On the 2 laſt Days, namely the third and eſpecially the ſixth of *December*, the Places of Mars, deduced from Obſervation, are doubtful; theſe therefore may be rejected.

The Places of the fixt Stars in the Scheme annexed, from the *Britannic Catalogue*, to the Beginning of the Year 1690, are taken without any Reduction: therefore to the Longitudes of Mars, exhibited by the Figure, muſt be added 39′ or 39′ 5″ for the Motion of the fixt Stars in 46 Years and about 10 or 11 Months.

The Obſervations of *November* 9 are omitted above, and I ſhall add them here, with the Place of Mars deduced from them.

Styl. nov.	True time Veſp.				Parts of the Micrometer	Value of Parts of the Microm.
	h ′					
Nov. 9.	9. 28.	♂ *a.*	Teleſ. 7 feet		25.	0. 10. 0.
	9. 30.	♂ *e* ♓	. . .		152.	1. 0. 48.
	9. 34.	♂ *c.*	. . .		136.	0. 54. 24.
	9. 41.	♂ *a.*	Teleſ. 9 feet		32 ½.	0. 9. 40.

Styl. nov.	True time Veſp.		Longitude of Mars.	Latitude of Mars.
			° ′ ″	° ′ ″
Nov. 9.	9. 34.	Obſervation.	♈ 13. 22. 20.	1. 1. 30. S.
		Manfredi.	13. 22. —	1. 0. 30.
		Ghiſler.	13. 11. —	1. 3. 30.
		Deſplaces.	13. 25. —	1. 2. 30.

An Obſervation of the Tranſit of Mercury *over the* Sun, Oct. 31. 1736, *by Mr* George Graham, *F. R. S. made in* Fleetſtreet, London. No. 446. p. 102. July, &c. 1737.

XXV. 1.

Apparent Time.

h	′	″	
9	22	00	*Mercury* not yet ſeen, then Clouds.
9	25	37	I firſt ſaw *Mercury* for a few Seconds, and judged he was got entirely within the *Sun*'s Diſk, or perhaps a little more; then Clouds again, with ſome Intervals of a few Moments between, which allowed us a Sight of *Mercury* about three or four ſeveral times; then quite cloudy till near 12, when we had a Sight of the *Sun* for a few Minutes, and took his *Tranſit* upon the Meridian; at which time we judged *Mercury*

h.	′	″	
			Mercury to be about two of his Diameters, or a little more, within the *Sun*'s Disk, and a little past the vertical Line.
12	10	27	We had again a Sight of the *Sun*, but *Mercury* was gone off.

2. The Sky was very clear, and the Air not disturbed by any Wind. *Roversius* happened to be the first who perceived the Planet at the edge of the Sun at $22^h\ 8'\ 37''$ *p. m.* and it's inner Contact with the Sun at $22^h\ 11'\ 12''$ we made use of Clocks regulated by a Meridian Line drawn by *Zanotti*, by equal Altitudes of the Sun in the Morning and Evening, taken several times.

—*Observed at* Bologna, *by* Eustachius Manfredi, *F. R. S.* Ibid. p. 103.

The Planet was perceived something later by other Observers in the Limb of the *Sun*. For my own part, I did not perceive it, with a Telescope of 11 feet, till $22^h\ 9'\ 5''$ when it had plainly entered the *Sun*, and I estimated it's inner Contact to be at $22^h\ 10'\ 53''$ But the former Observation is far more certain, as being made with a better Instrument. But since from the times of the Egress of the Planet, which will afterwards be mentioned, it is manifest that it's Body spent $3'\ 16''$ in going out, if we subduct so much from the time of the inner Contact observed by *Roversius*, the exterior Contact, or first Appulse of *Mercury* to the *Sun* will be still more certain, at $22^h\ 7'\ 56''$.

The subsequent Observations tended to find some Points of the Path, which the Planet was seen to describe in the *Sun*. We referred each of those Points to a Horary Circle, and also to a Parallel drawn through the Centre of the *Sun*, according to *Cassini*'s Method, marking the times by the Clock, at which the Limbs of the *Sun* and *Mercury* passed over the Horary Thread of the Micrometer. *Zanotti* obtained many of these Points with a Telescope of 8 feet; and I obtained 1 or 2 with a Tube of 6 feet, to which an excellent Micrometer was fitted, made by *Jo. Jacobus Marinonius. Roversius* and *Thomas Perellus, M. D.* determined some other Points with the same. Hither also belongs the Observation made by *Perellus* on the Meridian, with a mural Semi-circle, by which Observation the right Ascension of the Planet was found to be $11''\frac{1}{2}$ of Time greater, and the Declination $58''\frac{1}{2}$ of Time less than of the Centre of the *Sun*. Besides *Zanotti* took upon himself to describe the Positions of the more remarkable Spots, many of which were seen that Day in the *Sun*. It was easy to distinguish between the Planet and those Spots, because it was exactly round, and very black, and surrounded with no Ring.

Franciscus Algarottus, *F. R. S.* observed the Beginning of the Egress with a Telescope of 8 feet at $50'\ 1''$ *p. m.* the End at $53'\ 6''$. I observed the Beginning with a Telescope of 11 feet at $51'\ 7''$ the End at $53'\ 44''$. *Roversius* observed only the End with a Telescope of 14 feet at $54'\ 1''$; but these Observations are not very certain, because the Telescopes were but indifferent, and the Wind rising about that Time shook them

a little. Therefore we muſt prefer the Obſervation which was made with a Teleſcope of 22 feet, by *Franciſcus Vandellius*, Profeſſor of Military Architecture. He determined the inner Contact at 50′ 50″, and the outer at 54′ 6″, whence the Stay of the Planet in the Limb was 3′ 16″, and the Time of the Egreſs of the Centre 52′ 28″, which, according to my Obſervation, ſhould be 52′ 25″.

Thus much for the Obſervations themſelves; I ſhall now mention what I have deduced from comparing them with *Zanotti*. Aſſuming the Diameter of the *Sun* to be 32′ 34″, and the Time of it's paſſing thro' the Horary Circles 2′ 17″ (which Numbers are ſet down in the Tables of the Modern Aſtronomers, and confirmed by the Obſervations themſelves) we have ſet down thoſe Points by obſerving the Bounds of the planetary Path; and as an Account of the ſmall Fallacies of the Obſervations they would all fall exactly upon the ſame Right Line, we thought none more proper to reconcile them, than if we determined a Perpendicular Line drawn from the Centre of the *Sun* to the Path of the Planet, to contain an Angle of 23° 40′ to the Eaſt with the Horary Circle; and if we ſettled the Length of that Perpendicular from the Centre to the Path to be 13′ 58″ to the North. From theſe we have calculated all the reſt after the following manner.

h	′	″	
27	7	56	Fig. 80. Ingreſs of *Mercury* into the Diſk of the *Sun*.
22	9	34	Ingreſs of the Centre.
22	11	12	Total Ingreſs.
0	50	50	Beginning of the Egreſs.
0	52	28	Egreſs of the Centre.
0	54	6	Total Egreſs.
2	42	54	Stay of the Centre of *Mercury* in the Diſk of the *Sun*.
1	21	27	Half the Stay.
23	31	1	Time of the middle Tranſit.
°	′	″	
23	40	0	Angle of the Perpendicular Line to the Path of the Planet with the Horary Circle determined by the Obſervations, to the Eaſt.
105	48	0	Angle of the Eclipticк with the Horary Circle from the Aſtronomical Tables, to the Eaſt.
82	8	0	Is therefore the Angle of the Ecliptick with the Perpendicular to the apparent Path of *Mercury*.
7	52	0	Is alſo the Angle of the apparent Path with the Ecliptick.

Diſtance

°	′	″	
0	13	58	Diſtance of the Path from the Centre of the *Sun* to the North, found by Obſervations.
0	16	17	Semi-Diameter of the *Sun*.
0	16	45	Length of the Path within the *Sun*'s Diſk.
0	8	22	Half of it's Length.
0	6	10	Horary Motion of *Mercury* in the apparent Path.
0	6	6	Apparent Horary Motion in the Ecliptick.
0	1	58	Portion of the Path between the middle of the Tranſit and the Conjunction.
0	10	20	Portion of the Path from the Ingreſs to the Conjunction.
0	6	24	Portion of the ſame from the Conjunction to the Egreſs.
0	10	15	Difference of Longitude of *Mercury* and the *Sun* in the Ingreſs.
0	6	21	Difference of Longitude in the Egreſs.

h	′	″	
0	19	2	Time from the middle of the Tranſit to the Conjunction.
True Time			Time of the Conjunction at *Bologna*
23	50	3	
Mean Time			
23	34	25	

°	′	″	of *Scorpio*.
19	23	30	Longitude of the *Sun* and *Mercury* at the very Conjunction, by *Caſſini*'s Tables.
			This Longitude agrees within 4″ with the Obſervation made by *Petrus Lilius*, *J. V. D.* the ſame Day by the Meridian-Gnomon of *St Petronius*.
0	12	37	Latitude of *Mercury* in the Northern Ingreſs.
0	14	54	Latitude in the Northern Egreſs.
0	0	50½	Horary Motion into the Latitude.
0	14	1	Latitude in the Northern Conjunction.

h	′	″	
16	39	0	Space of Time from the Tranſit of *Mercury* thro' the aſcending Node to the Conjunction.
7	11	0	True Time } Time of the Tranſit thro' the Node.
6	55	0	Mean Time }

Motion

°	′	″	
4	15	47	Motion of *Mercury* seen in the Orbit out of the *Sun* at the Distance of 16h 39′ about this Time, or Argument of Latitude in Conjunction.
4	13	56	The same Motion reduced to the Eclip tick.

°	′	″ of *Taurus*	
15	9	34	Place of the ascending Node of *Mercury* seen out of the *Sun*.

Log.	449301	Distance of *Mercury* from the *Sun* at the Time of Conjunction by *Cassini*'s Tables.
Log.	499503	Distance of the Earth from the *Sun* by the same Tables.

°	′	″	
0	30	31	Latitude of *Mercury* in Conjunction seen out of the *Sun* North.
6	51	0	Inclination of the Orbit of *Mercury* to the Eclip tick.

h	′	″	
0	3	16	Time from the inner Contact of *Mercury* to the outer one in the Egress, by Observation.
°	′	″	
0	0	20	Portion of the Path gone through at this Time by *Mercury*.
58	50	0	Angle of the Path with the Semi-Diameter of the *Sun* in the Egress.
0	0	10	Apparent Diameter of *Mercury* as nearly as possible.

—*Observed at* Wittemburg, Nov 11. 1736. *by* J. Frid. Weidler, *F. R. S.* Ibid. p. 110. Fig. 81.

3. *Mercury* appeared within the *Sun*'s eastern Limb (as in the Scheme)

h	′	″		
10	49	20	at	1
11	36	0	☿ abt.	2
11	52	20	at	3
12	2	30		4
	4	30		5
	44	20		6
	52	45		7

A Transit of Mercury *under the* Sun Oct. 31. 1736, *by* J. Bevis, *M.D.* No. 471. p. 622. *Read* December 15, 1743.

XXVI. I went to *Greenwich*, *Oct.* 31. 1736, early in the Morning, to observe the Conjunction of *Mercury* with the *Sun*, being invited by Dr *Halley*, who condescended to assist me. The Sky was very clear at the rising of the *Sun*, but the Wind was very brisk. Dr *Halley* was in the same Room with me, and was pleased to attend the Clock, whilst I took the care of a Telescope of 24 feet. I began to observe about

2

about 8, being afraid of miſſing the Ingreſs, if there ſhould be any Error in the Calculation: But I could ſee nothing in the *Sun* beſides Spots. The Sky was preſently after covered with Clouds. About ten the Clouds opened a little, and gave me the firſt Opportunity of ſeeing *Mercury* under the *Sun*, which was taken away in a Moment by very thick Clouds. I had not waited long before I ſaw him again, and ſhewed him to Dr *Halley* on the Face of the *Sun*. Then came a long Succeſſion of dark Clouds. About Noon it began to be clear, and Dr *Halley* obſerved the *Sun* culminating with his great Mural Quadrant. I had now great Hopes of ſeeing the Egreſs of *Mercury*, and renewing my Application, made the following Obſervations.

	h	′	″	
Oct. 30.	23	50	45	The Centre of *Mercury* was 1′ 8″ diſtant from the *Sun* by the Micrometer.
31.	0	2	39	*Mercury* was diſtant from the *Sun*'s Limb about his own Diameter.
		7	4	The Centre was judged by the Eye to be gone out.
		8	33	The exterior Contact, the Sky being very clear.

XXVII. I have ſent you a Scheme of the Phaſe of the *Sun*, *Oct.* 31ſt, $11^h\ 5'\ 12''$ as taken by my Teleſcope, which is a very good one of 10 feet; but as I had neither Croſs-Hairs, Micrometer, or other exact Inſtruments, the Obſervation may not be very exact: Beſides, I had only a Glimpſe of the *Sun* for 7 or 8 Minutes.

No. 459. p. 645.

A Tranſit of Mercury *over the* Sun, Oct. 31. 1738, *by* John Huxham *M. D. F. R. S* Fig. 82.

XXVIII. *April* 21, 1740, I had an Opportunity to obſerve *Mercury*, then near his deſcending Node, tranſiting the *Sun*'s Diſk. Being advertiſed by Dr *Halley*'s Calculations, that the former Part of this Tranſit would be viſible in our Horizon, I was reſolved to obſerve it in the beſt manner I could, with thoſe few Inſtruments I was furniſhed with; which were only thoſe I had received from my Predeceſſor Mr *Iſ. Greenwood*, and are the ſame that are mentioned by the late Mr *Thomas Robie* * being a 24 Foot Teleſcope, another of 8 Foot, and a braſs Quadrant of 2 Foot *Radius*, fitted with teleſcopic Sights, and having Croſs-Hairs fixed in the *Focus* of the Glaſſes. All theſe I got in Readineſs, being the more deſirous to make this Obſervation, becauſe *Mercury* had never as yet been ſeen entering upon or going off the *Sun*'s Limb at his deſcending Node, and this *Tranſit* ought to be inviſible to *Europe*. The better to obſerve *Mercury*'s Ingreſs on the *Sun*, I determined to make uſe of my 24 Foot Tube, while an Aſſiſtant I had with me uſed that of Eight Foot: After which I propoſed, in order to find out his Path in the *Sun*, to obſerve the Paſſages of *Mercury* and the *Sun*'s Limbs by an horizontal and vertical Hair in the Teleſcope of the Quadrant; and

A Tranſit of Mercury *over the* Sun, April 21. 1740, *by Mr* John Winthorp, *Holliſian Prof. Math. and Aſtron. at* Cambridge *in* New-England. No. 471. p. 572. *Read* Nov. 3. 1743.

* *See* Vol. VI. p. 173.

I choſe

I chose rather to deduce *Mercury*'s Right Ascensions and Declinations by Calculation from hence, than to observe them immediately in the common way of placing one of the Cross-Hairs parallel to the Equator, &c. because, as the *Sun* was likely to be low before *Mercury* made his Entrance, Refraction would have caused considerable Errors in the Places of *Mercury* determined in this Manner. Having no Clock, I was obliged to make use of my Pocket-Watch, which I know to be a good one; and by this it was easy to distinguish Time to a Quarter of a Minute, which would have served pretty well for the Ingress of the Planet. But as it was by no means sufficient for those other Observations I designed to make, I procured another Watch, which shewed Seconds; and both these Watches I adjusted to the apparent Time, by several Altitudes of the *Sun* taken with the Quadrant before the *Transit* began; and by Altitudes taken the next Day, I found that the Watches had kept time exactly enough. I expected that the Centre of the Planet would enter upon the *Sun* at $5^h\ 2'$; but being apprehensive that he might be earlier than the Calculation, I, for some time before that, with my 24 Foot Tube directed to the *Sun*, kept my Eye fixed on that Part of his Limb where the Planet was to enter, as steadily as I could for the Wind, which then blew fresh. This Precaution was not needless; for, at $4^h\ 54'\ 59''$, I perceived that *Mercury* had made a Impression on the *Sun*'s Limb; by the Quantity of which I concluded, that almost $\frac{1}{4}$ of his Diameter might be entered. After I had beheld this very plainly about a Minute, a small Cloud covered the *Sun* near $3'$; which then clearing off, and the Sun shining very bright, as before, I had again a distinct View of the Planet, and saw much more than half his Body on the *Sun*. I continued to see him till $5^h\ 0'\ 40''$, at which Time he seemed to be gotten almost wholly within the *Sun*; for he appeared now very near round, though I could not yet discern the *Sun*'s Light behind him. By the shaking of the Tube, I unfortunately missed the Moment of his interior Contact with the *Sun*'s Limb, but am certain it could be but very little later than this; for I presently after saw him fairly within the *Sun*. Upon which, I repaired to my Quadrant; but this being at my Lodgings, at some Distance from the long Telescope with which I observed the Ingress, and which I had no Convenience for raising nearer Home, almost half an Hour slipped away before it was possible for me to begin my Observations. I began them as soon as I could, and continued them till Sun-set, excepting when I was interrupted by the Clouds; and I observed sometimes one and sometimes the other Limb of the *Sun*, as I found it most convenient. It will be needless, I suppose, to give a Detail of all the Observations I made; I shall therefore select Two or Three, which I look upon as most exact, and most suitable to my present Purpose. One was as follows:

	h	′	″
The *Sun* at the Horizontal — — — —	5	37	59
The *Sun* at the Vertical — — — — —	0	39	1
Mercury at the Vertical — — — — —	0	39	16
Mercury at the Horizontal — — — —	0	40	1

This Obſervation gave me the *Azimuth* and Altitude of *Mercury* at his Paſſage by the vertical Hair; from whence I computed his Right Aſcenſion and Declination, and from thence his Longitude and Latitude. The Method of obtaining which being ſufficiently known, I ſhall ſay nothing upon it, but only mention the Reſult of the Numbers, which was, that at $5^{h}\ 59'\ 16''$, when *Mercury* paſſed the Vertical, his Longitude was $12^{\circ}\ 43'\ 5''$ ♉; and the *Sun* being then in $12^{\circ}\ 42'\ 27''$ of that Sign, *Mercury* was in conſequence of the *Sun*'s Centre, $38''$, his Latitude at the ſame time being $15'\ 2''$ North. Another Obſervation was thus:

	h	′	″
The *Sun* at the Horizontal — — — —	6	47	37
The *Sun* at the Vertical — — — — —	0	48	17
Mercury at the Vertical — — — — —	0	48	25
Mercury at the Horizontal — — — —	0	49	24

From hence I concluded, that at $6^{h}\ 48'\ 25''$ *Mercury* was in Antecedence of the *Sun* $3'\ 57''$ with $14'\ 20''$ North Latitude. I made another Obſervation after this; but the *Sun* being then very near the Horizon, his Limbs were not well defined, ſo that I look upon this Obſervation as much preferable to that. I ſhall ſet down only two more, which were made about the middle between theſe two; and were made by the *Sun*'s upper Limb.

	h	′	″
The *Sun* at the Vertical — — — — —	6	6	56
Mercury at the Vertical — — — — —	0	7	8
Mercury at the Horizontal — — — —	0	8	42
The *Sun* at the Horizontal — — — —	0	9	45

The *Sun* at the Vertical — — — — —	6	17	18
Mercury at the Vertical — — — — —	0	17	29
Mercury at the Horizontal — — — —	0	18	26
The *Sun* at the Horizontal — — — —	0	19	32

At the former of theſe Obſervations, *viz.* $6^{h}\ 7'\ 8''$ I computed the Longitude of *Mercury* to be in $12^{\circ}\ 42'\ 17''$ ♉, which being taken from the *Sun*'s Place in $12^{\circ}\ 43'\ 35''$ ♉, leaves $1'$, $18''$ for the Difference of Longitude between the *Sun* and *Mercury*; and his Latitude was then

$14' 47''$. At the latter Obſervation, the Difference of Longitude was $1' 55''$, and the Latitude of *Mercury* $14' 42''$.

From theſe Places of *Mercury* it appears, that his Horary Motion in Longitude from the *Sun* was now $3' 58''$; according to which, if we ſuppoſe the central Ingreſs to have been at $4^h 57'$, we ſhall find the Difference of Longitude at that time $3', 20''$; and the Semi-diameter of the *Sun* being $15' 57''$, the Latitude of *Mercury* muſt be $15' 36''$. Now the Angle of *Mercury*'s viſible Way with the Ecliptic being, by the Theory of his Motion, $10° 23'$, we muſt conclude the former of the obſerved Latitudes about $4''$ too ſmall, and the latter as much too large;——an Error very inconſiderable in this kind of Obſervations. From theſe things we may gather by an obvious Computation, that *Mercury* was in Conjunction with the *Sun*, in reſpect of Longitude, at $5^h 47'$ with $14' 59''$ North Latitude; and that his neareſt Diſtance to the Centre of the *Sun* was $14' 44''$; and when he was at his neareſt Diſtance, the Difference of his Longitude from the *Sun*'s was $2' 39''$ which he paſſed over in $40'$ of Time, and conſequently arrived at the middle of his Courſe in the *Sun* at $6^h 27'$: Whence the Semi-duration of the central *Tranſit* was $1^h 30'$, and the End at $7^h 57'$, an Hour after Sun-ſet.

As to the Place of *Mercury*'s Nodes, the Inclination of his Orbit to the Ecliptic, and the other Elements of his Theory, I pretend not to determine any thing from ſo ſhort a Series of Obſervations as this. I content myſelf with the foregoing Determinations, which, I hope, are not far from the Truth, having taken all the Care I could, both in the Obſervations and Calculations.

Tranſit *of* Mercury *over the Sun*, Oct. 25. 1743, *in the Morning, obſerved at Mr* Geo. Graham's *Houſe in* Fleet-ſtreet. No. 471. p. 578. *Read* Nov. 3. 1743.

XXIX. 1. The Beginning could not be ſeen by reaſon of Clouds; but about $8^h 45'$ *Mercury* was ſeen (through a Reflecting Teleſcope three Foot *Focus*, magnifying about 50 times) about four or five of his Diameters within the *Sun*'s Limb.

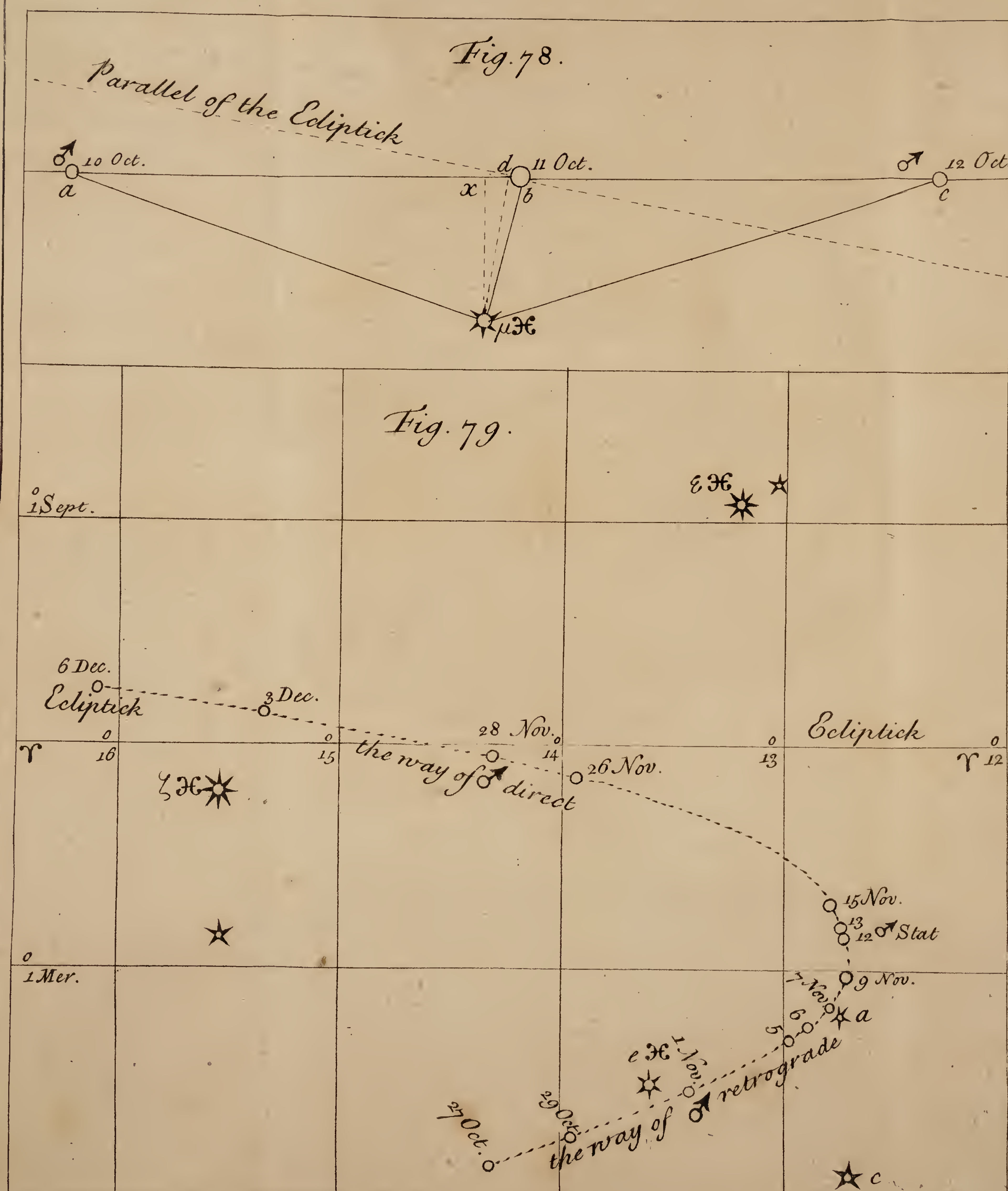
At Mr *Short*'s Houſe in *Surrey-ſtreet*, *Mercury* was ſeen juſt paſt the interior Contact $8^h 30' 59''$ through a Reflecting Teleſcope two Foot *Focus*, magnifying about 70 times; the Perſon who obſerved it ſays, that the Thread of Light between *Mercury* and the *Sun*'s Limb was ſo ſmall, as ſcarcely to amount to the 20th or 30th Part of *Mercury*'s Diameter.

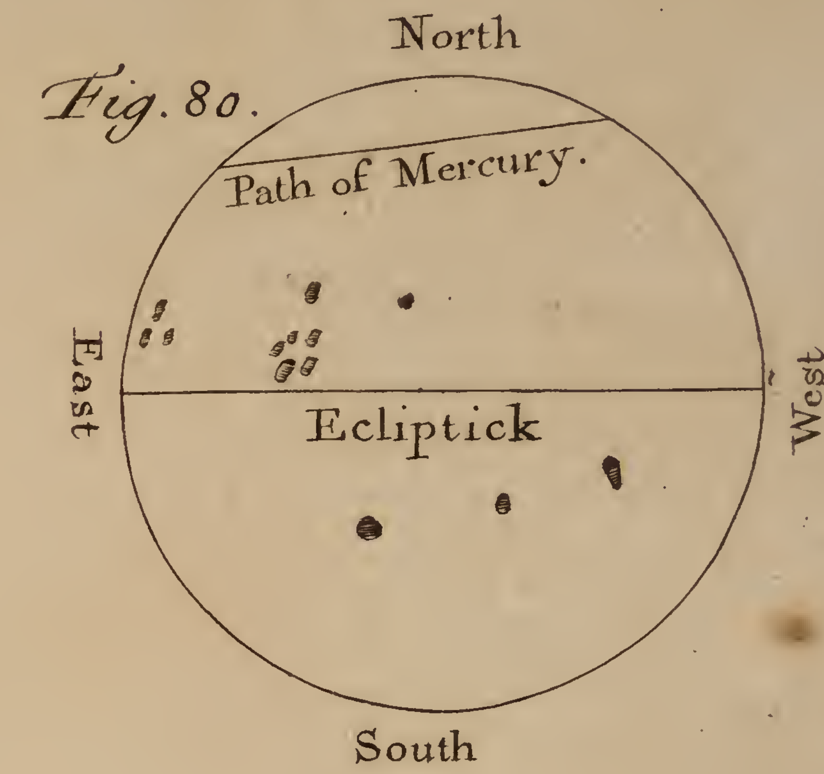
The following Differences of Right Aſcenſion between the Sun's preceding Limb and Mercury, *were taken at Mr* Short'*s Houſe.*

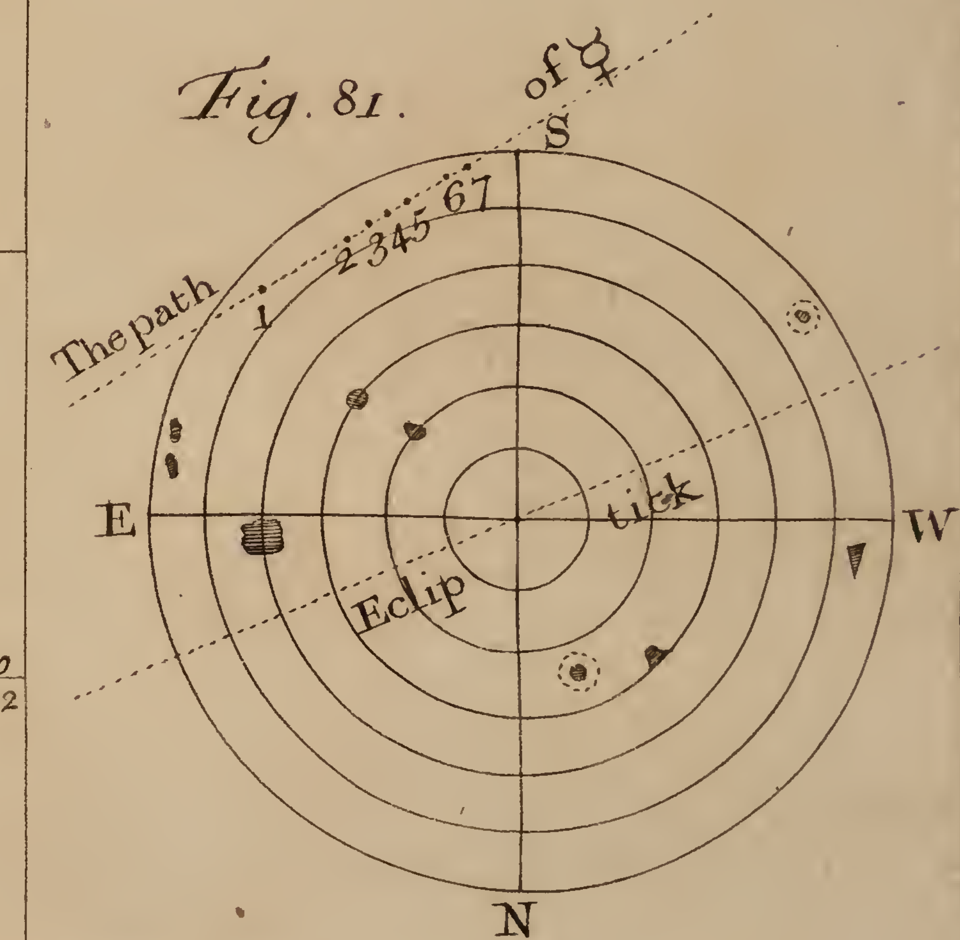
	h	′	″
Sun's preceding Limb touched the Wire at — —	10	58	55
Mercury touched the ſame Wire at — — — —	10	59	40
Sun's preceding Limb touched the Wire at — — —	11	48	4
Mercury touched the ſame Wire at — — — —	11	48	32
Sun's ſubſequent Limb touched the ſame Wire at — —	11	50	20

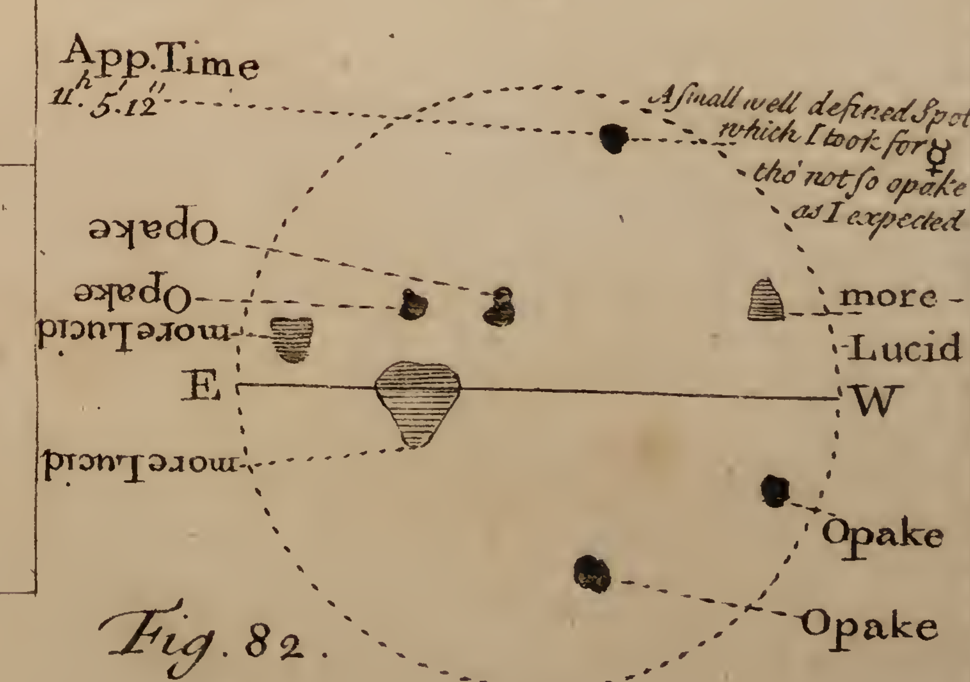
Sun's

Pl. XIV. Vol. VIII. part 1. pa. 202.









14

	h	′	″
Sun's preceding Limb touched the Wire at — —	11	49	20
Mercury touched the same Wire at — — — —	11	49	46
Sun's preceding Limb touched the Wire at — —	11	51	9
Mercury touched the same Wire at — — — —	11	51	36
Sun's preceding Limb touched the Wire at — —	12	1	33
Mercury touched the same Wire at — — — —	12	1	57

Mr *Graham* got an Observation madeby a Person in his Neighbourhood, by which it appears, that at $11^h\ 59'\ 50''$, *Mercury* preceded the Sun's Centre 42″ in Right Ascension.

The Sky clearing up towards one o'Clock, the following Times were observed at Mr *Graham*'s House with great Accuracy.

	h	′	″
Last interior Contact at — — — — — — —	1	0	42
End, or *Mercury* just leaving the Sun's Limb at — —	1	2	16

This last Observation agrees to a Second with the same Observation made by Dr *Bevis* at Mr *Sisson*'s House in the *Strand*.

During the Time of these Observations it blew a violent Gale of Wind, so that both Observers and Instruments were somewhat disturbed.

2. I made this Observation at *London*, in *Beaufort-Buildings*, situated about ½ a Minute West from the Royal Observatory. The Weather was the same as in my former Observation*, only the Wind blew harder, which caused a little shaking of the Telescope, tho' strongly Supported. I could not easily apply the Micrometer, and the single Observation made with it was found to be so inaccurate, by comparing it with a contemporary Observation made in a close Room at *Greenwich*, that I shall not mention it. Mr *Jer. Sisson* counted the Clock, whilst I observed the Sun. At 8 in the Morning nothing appeared in the Sun, and it was soon after obscured by many Clouds. At about 10 ½ I first discovered *Mercury*, having then finished almost half his Passage. The Sun was then covered with Clouds again, but at Noon grew bright, when the ascensional Difference of the Sun and *Mercury* appeared: for having placed three vertical Threads in the Focus of the meridional Passage. *Oct*, 24. $23^h\ 57'\ 48''$ T. App. the preceding Limb was come to the first of them, and in 25″ the Centre of *Mercury* came thither. It then grew very cloudy with Rain, so that I thought of giving over the Observation, but the Clouds breaking again, I proceeded, and had the Pleasure to see *Mercury* exceeding black upon the bright Body of the Sun, and set down the following Phases exactly.

—*by* John Bevis, *M. D*, No. 471. p. 624. *Read* Dec. 15. 1743.

* *Vide Supra*, §. XXVI.

Temp. Ap.	h	′	″	
Oct. 25.	0	58	34	The Diſtance between the Limbs of the Sun and *Mercury* nearly equal to the Diameter of *Mercury*.
	1	0	33	The laſt interior Contact.
		1	25	The Egreſs of the Centre, judged by the Eye.
		2	16	The laſt exterior Contact.

The Day before, a little before Noon the Diameter of the Sun was meaſured 32′ 27″ with an excellent Teleſcope of 12 Feet, armed with a Micrometer.

Mr *Bird* made a good Obſervation, about the Beginning of the Tranſit, with a catadioptrical Teleſcope that magnified much, in *Surrey-ſtreet* about 1″ ½ of Time Eaſt from the Place of my Obſervation. He perceived a very ſmall Thread of Light between the Limbs of the Sun and *Mercury*, which had juſt entered, ſcarce equal, as he ſaid to $\frac{1}{10}$ of the Diameter of *Mercury*, at 8^h 30′ 56″, that is, at 8^h 30′ 54″ ½ in *Beaufort-Buildings*, as appeared by an exact compariſon of the Clocks; wherefore I may venture to refer the total Ingreſs at *Beaufort-Buildings* to 8^h 30′ 40″ as near as poſſible.

I may therefore, from what has been ſaid, ſet down the whole Tranſit, as ſeen at *Beaufort-Buildings*, in the following manner.

Temp.Ap.	h	′	″	
Oct. 24.	20	28	57	The firſt exterior Contact.
		29	48 ½	The Ingreſs of the Centre.
		30	40	The firſt interior Contact.
25.	1	0	33	The laſt interior Contact.
		1	24 ½	The Egreſs of the Centre.
		2	16	The laſt exterior Contact.

—*by Mr* John Catlyn, No. 466. p. 235. *Read* Nov. 25. 1742.

3.

		h	′	″
The Equal Time of the true ☌ at *Greenwich* —	*Oct.* 24.	22	15	58
The Equation of Natural Days add — —			16	11
Apparent Time of the true ☌ — — —	*Oct.* 24.	22	32	9

At which Time the true Place of the Sun and of *Mercury* ſeen from the Earth — — —	m,	12	36	44
The Geocentric Latitude of *Mercury* — — —	South.		9	37
The Elongation in 5 Hours (*i. e.*) the 2 ½ immediately preceding and following the ☌			29	16
The Difference of Latitude in the ſame time — —			4	24
Therefore the Angle of the apparent Way of ☿ with the Ecliptic — — — — —		8	33	0
And the Diſtance of their Centres at the Time of their neareſt Approach — — — —			9	31
And the Motion of Interval between that and the ☌			1	26

And

	h	′	″
And the hourly Motion of *Mercury* in his Path over the Disk of the Sun — — — —		5	55 $\frac{1}{12}$
And the Motion of the ½ Duration from the first to the last exterior Contacts of the Limbs —		13	15
And the Motion of the same for the interior Contacts — — — — — — —		13	4
Hence the Time of the Interval from the 6 to the Middle — — — — — — —		14	32
of ½ the exterior *Transit* — — — — —	2	14	22
of ½ the interior *Transit* — — — — —	2	12	30

Hence

	h	′	″	
The first exterior Contact of the Limbs —	8	32	19	*Oct.* 25. Morning.
The first interior Contact — — —	8	34	11	
The nearest Approach of the Centres, or Middle — — — — — —	10	46	41	
The last interior Contact — — — —	0	59	11	Aftrenoon.
The last exterior Contact, or End of the *Transit* — — — — — —	1	1	3	

This Computation is made from Tables* which give the ascending Node of *Mercury* at the Time of this *Transit* 6′ 17″ too forward, according to the Result of very accurate Observations made of that in the Year 1723, by Dr *Halley*, Dr *Bradley*, and Mr *Graham*. Therefore making the Calculation with this Correction of the Place of the Node, the Times of the several Circumstances of the *Transit* will be as follows:

	h	′	″	
The first exterior Contact — — — —	8	29	21	*Oct.* 25. in the Morning.
The first interior Contact — — — —	8	31	5	
The nearest Approach of the Centres — —	10	46	6	
The last interior Contact — — — —	1	1	7	Afternoon.
The last exterior Contact — — — —	1	2	51	

This *Transit* may be very aptly compared with that which happened on the 24th Day of *October* 1697 †; as happening at the End of a remarkable Period in *Mercury*'s Motion, by which he is nearly in the same Situation, with respect to the Sun, at every Completion of it. Dr *Halley* in his Series of Moments, in which *Mercury* is joined to the Sun, *&c* ‖. makes the Middle of this *Transit* at 11′ past Six in the Morning the 24th Day, or the 23d Day at 18^h 11′ *p. m.* and the Distance of the Centres of the Sun and *Mercury* 10′ 4″.

It may not be amiss to examine and compare these Numbers by such Observations as were made of this *Transit*, and may be depended on, and thereby to collect the Difference between Computation and Observation; and whatever Error arises in Excess or Defect by a proper

* Vol. VI. Chap. III. §. 39. † Mean Period 46 Years 1^d 5^h 43′ 42″.

‖ Vol. I. Chap. IV. §. 100.

2 Application

Application to the *Transit* of 1743, it is imagined, will foretel it with a greater Degree of Exactness, than a Calculus from any Theory whatsoever.

There was only the Egress of *Mercury* in the *Transit* of 1697, capable of being observed in *Europe* *; which was done at *Nuremberg* in *Germany*, by Mr *Wurtzelbaur*, and at *Paris* by M. *Cassini*; at *Greenwich* Clouds prevented it. At *Nuremberg* Mr *Wurtzelbaur* observed *Mercury* to go off of the Disk of the Sun † at $8^h\ 45'\ \frac{1}{2}$ *mane* about $73\ \frac{1}{2}$ Degrees from the Vertex of the Sun to the Right-Hand; and M. *Cassini* observed the same accurately at $8^h\ 10'\ 24''$ *mane*; therefore from the known Difference of Meridians of these Places, the Egress must have happened at *Greenwich* at $8^h\ 1'$ *mane*.

The Observation of Mr *Wurtzelbaur* will greatly avail at coming at the Duration of the *Transit*. It is mentioned, that *Mercury* left the Limb of the Sun $73^\circ\ 30'$ from his Vertex to the Right. Now at that time at *Nuremburg*, the Angle of the Ecliptic with the Vertical passing through the Sun's Centre, was $42^\circ\ 3'\ 5''$; therefore the last Point of Contact on the Sun's Limb was observed $31^\circ\ 26'\ 55''$ from the Ecliptic to the South, and consequently his Latitude was $8'\ 28''$ South at that time.

To find the Point on the Sun's Limb of the Ingress, in order to come at the Duration of the *Transit*, we must be beholden to Computation, and the Theory of *Mercury*'s Motion: I have therefore, from the Tables from which the above Times of the *Transit* of 1743 are drawn, carefully computed his Motion along his Path crossing the Disk of the Sun, and find that he moved along it after the Rate of $5'\ 53''\ \frac{1}{4}$ in an Hour, and the Difference of Latitude in 5 Hours $4'\ 21''$, and his Elongation $29'\ 7''$: Therefore the Angle of his visible Way was $8^\circ\ 29'\ 50''$, which, doubled, and added to $31^\circ\ 26'\ 55''$, gives $48^\circ\ 26'\ 35''$, his Distance, on the Limb of the Sun from the Ecliptic also to the Southward at his Ingress on it; therefore the nearest Approach of his Centre to that of the Sun was $10'\ 19''$, and the Length of the Path run during the *Transit* $25'\ 14''$, and consequently the time of running it $4^h\ 17'$ the half of which $2^h\ 8'\ \frac{1}{2}$, subtracted from $20^h\ 1'$, the End of the *Transit* at *Greenwich*, gives the Middle there at $17^h\ 52'\ 30''$ earlier by $18'\ \frac{1}{2}$ than the Series of Moments, *&c.* give it.

Now as the said Series makes the Middle of the *Transit* of 1743, at $11^h\ 2'$ *mane*, and as it corresponds with that of 1697; and the Computation of that is $18'\ \frac{1}{2}$ too late by the Series of Moments, *&c.* it may be reasonably expected, that the same Computation for this of 1743 will be so much too late too; and if so, the Middle may be put down at $43'\ \frac{1}{2}$ past 10, or $44'$ at farthest, *October* 25th in the Forenoon.

* *Flamstead*'s *Hist. Cœlest.* Lib. II. Fol. 32. † Vertex to the Right, it says, *a Nadir Solis ad dextras*; but it is a manifest Mistake, as any one upon Trial may find.

By

By Computation from the Tables above-mentioned, with the Correction of the Node, I make the Diſtance of the Centres at the neareſt Approach in 1697, to be 10′ 33″, but by the Obſervations of Mr *Wurtzelbaur* it turns out only 10′ 19″, leſs by 14″. Should therefore their Diſtance in 1743 computed in the ſame manner at 9′ 10″ be as much diminiſhed, the Duration of the *Tranſit* will be protracted no leſs than 5′ 24″, and the firſt Contact will be 2′ 42″ earlier, and the laſt ſo much later, than the Times abovementioned for them.

N. B. In the Computation of the *Tranſit* of 1743, the Semidiameter of the Sun is ſuppoſed 16′ 14″ ½ and that of *Mercury* 4″ ½; but in that of 1697, have taken *Mercury*'s only 3″ ½, imagining the preciſe Moments, of the firſt and laſt exterior Contacts are not obſervable; but that the Ingreſs is ſeen ſome little Time later, and the Egreſs ſooner, than the true Times thereof. I have all along ſpoke of the Motion of *Mercury* without mentioning that of the Sun, whereas, in Reality, it is that of them both jointly; but as we may ſuppoſe the Sun to ſtand ſtill during the *Tranſit* it will then be conſidered as the apparent Motion of *Mercury* alone for that Time.

XXX.

An Occultation of Mercury *by* Venus, May 17. 1737. *at the Obſervatory at* Greenwich, *by* J. Bevis *M.D.* No. 450. p. 394. Oct. &c. 1738. Fig. 83.

Apparent Time.

h	′	″	
1	37	3	The preceding Limb of *Venus* paſſes the Meridian, the Centre from the *Vertex* 25° 46′ 35″; but I could not ſee *Mercury* within the Teleſcope.
9	4	9	The Centre of *Mercury* preceded the preceding Limb of *Venus* 12″ of Time.
	6	20	The ſame preceded, as before, the ſame Quantity of Time.
	28	0	As *Mercury* ran along the parallel Thread of the Micrometer, the ſouthern Cuſp of *Mercury* was cut by the ſame Thread ☽ ☿, whence I gathered that *Venus* would cover *Mercury*, or at leaſt touch him; therefore I drew out the Micrometer, that I might diſcern the inner Contact the better, with a Tube of 24 Feet.
	43	4	*Mercury* is not diſtant from *Venus* more than $\frac{1}{10}$ or $\frac{1}{12}$ of the Diameter of *Venus*: then interpoſing Clouds.
	51	10	*Venus* ſhines out again very bright, but all *Mercury* lies hid under *Venus*: The Clouds now cover *Venus* again, hindering any farther Contemplation of ſo rare a Spectacle.

May 18. *p. m.* Meridian Diſtance of the Sun from the *Vertex* 30° 4′.

h	′	″	
1	31	53	The preceding Limb of *Venus* paſſes the Meridian. The Centre diſtant from the *Vertex* 25° 57′ 15″.

 I could

I could not see *Mercury* culminating this Day, tho' the Sky was very clear.

N. B. The Distances from the *Vertex* are not cleared from the Refractions.

An Observation on the Planet Venus, (with regard to her having a Satellite) made by Mr James Short, F.R.S. at Sunrise, Oct. 23. 1740. No. 459. p. 646. Jan. &c. 1741.

XXXI. Directing a Reflecting Telescope of 16.5 Inches Focus, (with an Apparatus to follow the diurnal Motion) towards *Venus*, I perceived a small Star pretty nigh her; upon which I took another Telescope of the same focal Distance, which magnified about 50 or 60 times, and which was fitted with a Micrometer, in order to measure it's Distance from *Venus*; and found it's Distance to be about 10°. Finding *Venus* very distinct, and consequently the Air very clear, I put on a magnifying Power of 240 times, and, to my great Surprize, found this Star put on the same Phasis with *Venus*. I tried another magnifying Power of 140 times, and even then found the Star under the same Phasis. It's Diameter seemed about ⅓, or somewhat less, of the Diameter of *Venus*; it's Light was not so bright or vivid, but exceeding sharp and well defined. A Line, passing through the Centre of *Venus* and it, made an Angle with the Equator of about 18 or 20 Degrees.

I saw it for the Space of an Hour several times that Morning; but the Light of the Sun increasing, I lost it altogether about a Quarter of an Hour after Eight. I have looked for it every clear Morning since, but never had the good Fortune to see it again.

Cassini, in his Astronomy, mentions much such another Observation.

I likewise observed Two darkish Spots upon the Body of *Venus*; for the Air was exceeding clear and serene.

Several Astronomical Observations made at Pekin, by the Jesuits. No. 468. p. 306 Jan. 1742-3.

XXXII.

1740.		h	′	″		
Nov.	8.	18	34	0	*p. m.*	♀ preceding the Star η in ♍ was more West in the right Ascension 2′ 54″ of Time, and more North in the Declination 6′ 30″.
	9.	18	34	0	*p. m.*	♀ following yesterday's Star, was more East in the right Ascension 1′ of Time, and more South in the Declination 15′ 40″ Distance 21′ 53″.
	22.	18	43	0	*p. m.*	♀ followed the Star ϑ in ♍ in right Ascension 4′ 27″ of Time, and was more southern in the Declination 13′ 20″.
	26.	7	45	0	*Vesp.*	The Star τ in ♒ stood in the Line of Dichotomy of ☽, from the southern Cusp in the Declination more southern 13′ 0″.

Dec.

		h	′	″		
Dec.	2.	17	20	0	*p.m.*	The Star ε in ♉ was above ☽ in the same right Ascension with the Centre of Plato, more North in the Declination 12′ 20″, Plato was distant from the northern Limb of ☽ 4′ 0″.
	4.	12	26	0	*p.m.*	☽ covered the Star η in ♊, which immerged against *Byrgius*; the Emersion was not observed because of a Cloud.
1741 *Jan.*	1.	7			*mane*	♀ distant from the Star ν in ♏ ♐ 34′. It followed it in right Ascension 1′ 50″ of Time: more North in the Declination 19′.
		5	59	30	*p.m.*	The western Limb of ☽ at the horary Thread in the Telescope.
		6	0	24	——	♃ at the Day-Thread, was distant from the northern Limb of ☽ 13′.
			1	15	——	The eastern Limb of ☽ at the same Thread.
		6	5	36	——	The western Limb of ☽ again at the same horary Thread.
			6	18	——	♃ at the same Thread, distant from the North Limb 12′ 30″.
			7	55	——	Eastern Limb of ☽ at the same Thread.
		11	34	0	——	♃ culminated, Altitude 73° 26′.
			43	0	——	☽ culminated, Altitude of the Centre 73° 15′.
	21.	5	30	0	*p.m.*	♂ preceded the Star of yesterday *c* in ♊ 1′ 8″ of Time in right Ascension, it was more southern in declination 5′.
	22.	5	15	0	*p.m.*	♂ preceding the Star of yesterday 2′ 45″ of Time in right Ascension, more southern in Declination 2′.
	28	7	18		*Vesp.*	♃ was distant from the Edge of ☽ 19′ 50″.
Feb.	22.	11	44	26	*p.m.*	The Moon covered the Star η in ♉ standing in a right Line with *Manilius* and *Censorinus*. The Emersion could not be seen.
	24.	9	27	0	*p.m.*	The Star η in ♊ below the ☽ stood in a Right Line with *Tycho* and *Plato*, being distant from this to the South 11′ 20″.
		13	38	45	*p.m.*	The Star μ in ♊ was covered by ☽ in a right Line thro' Tycho and Posidonius; which did not emerge before 13h 55′ when ☽ set behind a House.

		h	′	″	
Apr.	20.	10	50	42 *p. m.*	☽ covered the third Satellite, which was to the West of ♃.
		10	56	45	☽ touched the Limb of ♃ $10^h\ 57'\ 35''$ was the full immersion of him in the middle between each Cusp of ☽ directly toward the Centre. The other Satellites were not very discernable, because of the Atmosphere, and the Moon hid itself soon after behind the Houses.
Sept.	24.	8	7	15 *p. m.*	☽ covered the preceding Star of the Quadrangle before the southern Tail of the Whale, which just emerged at the rising of *Cleostratus.*
		9	0	13	The same emerged very near *Berosus.*

Observations upon the Comet that appeared in Jan. Feb. *and* March 1737, *made at* Oxford, *by* J. Bradley, *F. R. S. Sav. Prof. Astron.* No. 446. p. 111. July &c. 1737.

XXXIII. 1. I made several Observations on the late Comet, during the last 5 Weeks of it's Appearance, which enabled me to find out the Elements of a Parabolic Trajectory, upon which a *Calculus* might be founded, that would correspond with each of my Observations within about 1′ of a Degree: But the first of them being taken many Days after the Time of the *Perihelion*, and the whole Series comprehending but a very small Portion of the Trajectory; I was sensible, that a little Error, either in the Observations themselves, or in the Places of the Fixt Stars with which the Comet was compared, might occasion a considerable Difference in the Situation and Magnitude, &c. of the Orbit deduced from them alone; and therefore I was desirous of having some earlier and accurate Observations, in order to determine those Elements with more Certainty: But I have not yet been able to procure them.

I first saw the Comet *Feb.* 15th 1737, between 6 and 7 in the Evening, when it's *Nucleus* appeared small and indistinct, and it's Tail (extending above a Degree from the Body) pointed towards the Star *in Lino Austral. Piscium*, marked ξ by *Bayer.* Applying my Micrometer to a good 7 Foot Tube, I observed, that at $7^h\ 32'$ *Temp. Æquat.* the Comet preceded the said Star $1^\circ\ 1'\ 40''$ in Right Ascension, and was $20'\ 20''$ more Southerly than the Star. *Note*, That the equal Time is likewise made use of in all the following Observations.

Assuming the Place of this Star, as it is settled in *the British Catalogue*, (as I shall likewise the Places of others hereafter mentioned) it follows, that the Comet's Right Ascension was $23^\circ\ 58'$, and it's Declination $1^\circ\ 31'\ 55''$, North.

Feb. 17. $7^h\ 33'$ the Comet followed α *in Nodo Lin. Piscium* $31'\ 25''$ in Right Ascension, and was $52'\ 30''$ more Northerly. Hence the Comet's Right Ascension was $27^\circ\ 38'\ 20''$ and it's Declination $2^\circ\ 21'\ 10''$ North.

Feb.

Feb. 18. 7^h 14′ a ſmall Star (whoſe Right Aſcenſion was afterwards found to be 29° 0′ 5″ and Declination 2° 58′ 30″ North) preceded the Comet 24′ in Right Aſcenſion, and was 15′ 30″ more Northerly. Hence the Comet's Right Aſcenſion was 29° 24′ 5″, and it's Declination 2° 34′ North.

Feb. 21. 7^h 25′ the Comet preceded ν *Ceti* 1° 6′ in Right Aſcenſion, and was 38′ 20″ more Southerly. Hence it's Right Aſcenſion was 34° 25′ 10″, and it's Declination 3° 47′ 20″ North.

Feb. 22. 7^h 45′ the Comet followed ν *Ceti* 30′ 5″ in Right Aſcenſion, and was 18′ 45″ more Southerly. Hence the Comet's Right Aſcenſion was 36° 1′ 15″, and it's Declination 4° 6′ 55″ North.

Feb. 25. 7^h 45′ a ſmall Star (whoſe Right Aſcenſion was afterwards found to be 40° 34′, and Declination 5° 5′ 30″ North) followed the Comet 2′ 30″ in Right Aſcenſion, and was 2′ 30″ more Northerly than the Comet. Hence the Comet's Right Aſcenſion was 40° 31′ 30″, and it's Declination 5° 3′ North.

The Difference of Right Aſcenſion and Declination between this Star and the Comet was taken with a 15 Foot Teleſcope; but the Place of the Star was determined by one Obſervation made with the 7 Foot Tube.

Feb. 27. 8^h 45′ the Comet preceded a ſmall Star 1° 16′ in Right Aſcenſion, and was 2′ 15″ more Southerly. The Right Aſcenſion of this Star was afterwards (by a ſingle Obſervation) found to be 44° 37′ 40″, and it's Declination 5° 38′ 30″ North. Hence the Comet's Right Aſcenſion was 43° 21′ 40″, and it's Declination 5° 36′ 15″ North.

March 4. 8^h a ſmall Star (whoſe Right Aſcenſion was found to be 49° 30′ 30″, and it's Declination 6° 38′ 30″ North) preceded the Comet 7′ 30″ in Right Aſcenſion, and was 10′ more Southerly. Hence the Right Aſcenſion of the Comet was 49° 38′, and it's Declination 6° 48′ 30″.

March 12. 8^h 25′ the Comet preceded μ *Tauri* 2° 5′ 50″ in Right Aſcenſion, and was 4′ 25″ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 58° 12′ 40″, and it's Declination 8° 16′ 50″ North.

March 14. 9^h the Comet followed the *47th Star of* Taurus *in the* Britiſh *Catalogue* 12′ 50″ in Right Aſcenſion, and was 15″ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 60° 8′ 5″, and it's Declination 8° 34′ 5″ North. This, and all the following Obſervations, were made with a good 15 Foot Teleſcope, the Comet now appearing too faint to be well obſerved with the 7 Foot Tube.

March 17. 8^h 40′ the Comet followed ϒ *Tauri* 25′ 5″ in Right Aſcenſion, and was 9′ 40″ more Northerly. Hence it's Right Aſcenſion, was 62° 47′ 55″, and it's Declination 8° 58′ 45″ North.

March 19. 7^h 50′ the Comet followed the ſame Star 2° 4′ 50″ in Right Aſcenſion, being 23′ 55″ more Northerly. Hence it's Right Aſcenſion was 64° 27′ 40″; and Declination 9° 13′ North.

 The

The fame Night, at 9^h the Comet preceded *d Tauri* 47′ 40″ in Right Afcenfion, and was 22′ 50″ more Southerly. Hence it's Right Afcenfion was 64° 30′ 20″, and Declination 9° 12′ 35″ North.

March 20. 8^h 5′ the Comet preceeded *d Tauri* 30″ in Right Afcenfion was 16′ 35″ more Southerly than the Star. Hence it's Right Afcenfion was 65° 17′ 30″, and Declination 9° 18′ 50″ North.

March 22. 8^h 15′ the Comet followed the fame Star 1° 36′ 10″ in Right Afcenfion, and was 3′ 50″ more Southerly. Hence it's Right Afcenfion was 66° 54′ 10″, and Declination 9° 31′ 35″ North.

This was the laft Night that I faw the Comet; for the Moon being then in her Increafe, entirely obftructed it's further Appearance. The Light of the Comet was indeed (even in the Moon's Abfence) fo very weak, that I found it difficult, in fome of the latter Obfervations, to take it's Place with any tolerable Certainty; which is, in part, the Caufe of fome little Difagreement obfervable in the Comet's Places taken from the fame Stars on different Nights; though there are likewife other Irregularities that occur in this Series of Obfervations, which feem to arife from fmall Errors in the affumed Places of the Fixt Stars.

Suppofing the Trajectory defcribed by this Comet to be nearly *Parabolical*, conformable to what Sir *I. Newton* has delivered*. I collect from the foregoing Obfervations, that the Motion of this Comet in it's own Orbit was *direct*, and that it was in it's *Perihelion*, *Jan.* 19. 8^h 20′ *Temp. Æquat. Lond.* That the Inclination of the Plane of the Trajectory to the Ecliptick was 18° 20′ 45″. The Place of the Defcending Node ♉ 16° 22′. The Place of the *Perihelion* ♒ 25° 55′. The Diftance of the *Perihelion* from the Defcending Node 80° 27′. The Logarithm of the *Perihelion Diftance* from the Sun 9.347960. The Logarithm of the Diurnal Motion 0.938188.

From thefe Elements (by the Help of Dr *Halley*'s general Table for Comets, to which they are adapted) I computed the Places in the following Table; which alfo contains the Longitudes and Latitudes of the Comet, calculated from the obferved Right Afcenfions and Declinations above-mentioned, together with the Differences between the obferved and computed Places.

* Princip. Math. Lib. iii.

Oxon.

Oxon. 1737. Temp. Æquat.	Com. Longit. Observat.	Lat. Aust. Observat.	Com. Longit. Computat.	Lat. Aust. Comput.	Diff. Long.	Diff. Lat.
Day h ′	s ° ′ ″	° ′ ″	s ° ′ ″	° ′ ″	″	″
Febr. 15 7 32	♈ 22 45 7	7 53 27	♈ 22 45 00	7 53 1	+ 7	+ 26
17 7 33	26 30 30	8 27 21	26 30 44	8 28 6	— 14	— 45
18 7 14	28 18 14	8 44 20	28 17 46	8 43 57	+ 28	+ 23
21 7 25	♉ 3 26 34	9 26 50	♉ 3 26 53	9 26 46	— 19	+ 4
22 7 45	5 4 53	9 40 00	5 5 28	9 39 27	— 35	+ 33
25 7 45	9 42 18	10 12 21	9 41 19	10 12 22	+ 59	— 1
27 8 45	12 36 43	10 31 42	12 36 16	10 31 13	+ 27	+ 29
Mar. 4 8 00	19 3 00	11 6 46	19 3 5	11 7 8	— 5	— 22
12 8 25	27 49 58	11 43 3	27 49 53	11 43 19	+ 5	— 16
14 9 00	29 47 42	11 49 59	29 47 19	11 49 26	+ 23	+ 33
17 8 40	♊ 2 30 57	11 56 31	♊ 2 30 50	11 56 49	+ 7	— 18
19 7 50	4 12 36	12 00 19	4 12 45	12 00 47	— 9	— 28
9 00	4 15 11	12 1 12	4 15 13	12 00 52	— 2	+ 20
20 8 5	5 3 10	12 3 5	5 3 32	12 2 33	— 22	+ 32
22 8 15	6 41 30	12 6 15	6 41 19	12 5 42	+ 11	+ 33

From the small Differences between the Comet's observed and computed Places, exhibited in the two last Columns of this Table, we may reasonably conclude, that the Orbit, as above determined, cannot differ much from the Truth, and must therefore be near enough to enable future Astronomers to distinguish this Comet upon another Return, and thereby to settle it's Period; which I cannot at present pretend to do, not having met with an Account of any former Comet that seems likely to have been the same with this, whereof a Description has been given particular enough to determine this Point.

On Mount Aventine, *at* Rome, *by the Abbot* Didacus de Revillas, *F. R. S.* Ibid. p. 118.

2. *Feb.* 16. about 7^h *p. m.* the Comet first appeared to us in the western Part of the Heavens, 8° or 9° lower than *Venus*; and declining a little from her vertical Circle toward the South. With the naked Eye we saw only a whitish Line, shining with a doubtful Light. But with an excellent Telescope of 6 Feet, besides the Tail, which was extended into the Part turned from the Sun, and appeared like a Line without the Telescope, we saw the *Nucleus*, though covered on all Sides with a thin Atmosphere. As no Quadrant was at Hand, and not only a Cloud intercepted the View of the nearest fixed Star, but the Twilight also concealed them, the apparent Place of the Comet could not be determined that Night.

From the 16th to the 19th, and also after the 25th there were many other Obstructions, which hindred us from observing. But in

in the Nights between the 19th and 26th, we could not accurately determine the apparent Place of the Comet, any otherwiſe than by comparing it's *Phænomenon* with *Venus*, becauſe we uſed only a ſmall Quadrant, of which the Optical Tube was ſcarce equal to an Engliſh Foot. Therefore from the vertical Altitudes both of the Comet and of *Venus*, obſerved at the ſame Time, we collected the vertical Differences of both, as follows.

Day	*p. m.*		*Vert. diff.*	
	h	′	°	′
20	7	59	5	22
22	7	00	3	56
23	7	20	3	13
24	6	15	2	30
25	7	30	1	47

The Tail of the Comet on the 22d, paſſing over the vertical Thread of the Micrometer, impended 1′ 7″. The Micrometer was fitted to the abovementioned Teleſcope.

24 8 03 Venus and the Comet appeared under an Angle of 7° 35′
25 7 50 They appeared under an Angle of — — — 8 05

—*at* Philadelphia *in* Penſylvania, *by Dr* Kearſly, *Ibid.* p. 119.

3. *January* 27, about Six in the Evening, I ſaw a dull Star about 3 or 4 Degrees above *Mercury*, and a little to the Southward of a Vertical paſſing through ☿, but took little Notice of it then, not thinking of a Comet; but by comparing ☿'s Place with the Fixt Stars, I afterwards thought it might be a Comet.——On the 31ſt, about 6h 30′ *p. m.* I took it's Diſtance from *Venus*, by a Reflecting Inſtrument of Mr *Hadley*'s Make, 14° 40′, but by a Foreſtaff, 14° 50′, and a Right Line paſſed over the Comet, *Venus*, and the *Pleiades.* The Night following, about 6h 20′ it's Diſtance from *Venus* was, by Mr *Hadley*'s Inſtrument, 13° 25′. The reſt of my Obſervations, by ſuch Inſtruments as I had, being none of the beſt, and the Comet's growing very dull, are as follow:

	h	′	*p. m.*
Feb. 7. — — —	6	47	Comet from *Venus* 7° 40′.
	7	3	——from *Aldebaran* 59° 40′. ——from *Algeneb* 17° 45′ by a Foreſtaff. A Right Line from the Comet over *Venus* paſſed over the bright Star in the Side of *Perſeus.*
11. — — —	7	14	Comet from *Venus* 7° 12′.
	7	20	A Right Line over the Comet, *Venus*, and Head of *Caſſiopeia.*
17. — — —	7	20	The Comet was in a Right Line, and to the Northward of two Stars; Diſtance of the Stars I ſuppoſed

	h ′	
		ſuppoſed to be about 40′, and the Comet from the leaſt 30′. Theſe Stars, I think, were the South Node of *Piſces*, brighteſt from *Venus* 10° 20′ from *Aldebaran* 50° 30′ as I found it ſet down, but muſt be very falſe.
Febr. 20.	— —	No Star within Sight of the Comet by the Teleſcope.
	30 7	Comet from *Aldebaran* 34° from *Lucida Cap.* ♈ 19½.
21. about	—— 30 8	Wanted about a Degree of *Oculus Ceti.*—— Which was the laſt Sight I had of it.

4. The Comet was firſt perceived about *Jan.* 26, but muſt, by it's Plainneſs then, have been viſible for ſome Time before. It was in the Weſt firſt of all, ſome Degrees below and directly under *Venus*: Every Night it appeared nearer to that Star, but inclined Northerly. In about a Fortnight, it was parallel to it, and in a Week after, was no more to be ſeen.

—*at* Spaniſh Town *in* Jamaica, *by* Roſe Fuller, *M. D.* *F. R. S. Ibid.* p. 122.

5. *Feb.* 9. for 7 Days laſt paſt, about 7ʰ *Veſp.* there hath appeared a dim Comet, as we took it to be: It is ſeen in the Weſt, under *Venus* towards the S. W. It looks through a Tube of 10 or 11 Feet long, like a dim or pale Planet; it's Tail tends upwards.

—*at* Madras, *by Mr* Sartorius, *a Miſſionary, ibid.*

6. *Jan.* 29. 1736-7, at 6ʰ 49′, *p. m.* we ſaw a Comet with a long bruſh Tail, at which Time it's Altitude was found 5° 15′, it's Diſtance from *Venus* 18° 5′; and *Venus*'s Altitude was obſerved 20° 40′. It bore due Weſt.

—*at* Liſbon, *by* G. R. Vanbrugh, *Eſq;* *on board the* Burford *Man of War.*

XXXIV. The Motion in it's own proper Orbit was retrograde.

The parabolic Orbit *for the* Comet *of* 1739, *obſerved by Seignor* Euſtachio Zanotti *at* Bologna. No. 461. p. 809, Aug. &c. 1741.

		°	′
The *Perihelion* was in — — — —	♋	5	11
The deſcending Node in — — —	♈	25	18
The *Perihelion* from the Node — — —		69	53

		d	h	′
The Comet was in the *Perihelion* — —	*June*	9	9	59
in the deſc. Node	*July*	18	4	57

The *Perihelion* of the Comet's Orbit was within the Sphere of the Orbit of *Veuns*, and without that of the Orbit of *Mercury*; being diſtant from the Sun 0,69614 Parts of the Earth's mean Diſtance from the Sun.

The Plane of the Orbit ſtood inclined to the Plane of the Ecliptic in an Angle of 53° 25′.

The Diurnal mean Motion, according as it is interpreted by Dr *Halley* in his Elements of Cometical Aſtronomy, was 1°,5707.

Observations on a Comet, by F. Frantz, a Jesuit at Austria, Feb. 1743. No. 470. p. 457. *Read* April 21, 1743.

XXXV.

Styl. Nov.

Feb. 11. *Vesp.* The Comet in a right Line with { ε & ζ of Ursa / α & δ Major

12. — — — — { γ of Ursa Major & λ of Draco, / δ & χ of Ursa Major.

13. — { in a right Line with χ of Ursa Major & λ of Draco, / in a rectangular Triangle with ψ & ω of Ursa Major.

14. almost in a right Line with { α of Leo & ν of Ursa Major, / β of Leo & β of Virgo.

15. { almost in a right Line with β of Leo & β of Virgo / in a rect. Triangle with ν & ξ of Ursa Major.

18. — — almost in a right Line with β of Leo & β of Virgo; at which Time the Comet & β of Virgo were almost equally distant from β of Leo.

21. — In the Tail of Leo near a little Star of the 6th Magnitude, which constitutes almost a right Line with { δ & β of Leo, β of Leo, & π of Virgo.

On the other Days it could not be observed with any Telescope, therefore the Comet was last seen at Vienna, near the abovementioned Star, of which the Long. 13° 16′ 28″ ♍ and N. Lat. 17° 30′ the Comet declined from this Star *Feb.* 21. 8ʰ 8′ 22″ towards the N. 28′ 16″ more to the W. 1° 15′ 12″.

Some Conjectures concerning the Position of the Colure *in the ancient* Sphere; *by the Rev.* Ebenezer Latham, *M. D.* No. 466. p. 221. Nov. 1742. Fig. 84.

XXXVI. I send you a Draught of the Constellation *Aries*, as it was exactly copied by Dr *White*, from a Book in the fine Library of *Samuel Saunders*, Esq; *F. R. S.* I do not know whether it may not be esteemed of some Moment towards the determining the famous Controversy with respect to Sir *I. Newton*'s Chronology. Dr *Halley* observes*, ‘ That the Dispute is chiefly, Over what Part of the ‘ Back of *Aries* the *Colure* passed. Sir *I. Newton* takes it to be over ‘ the Middle of the Constellation; *P. Souciet* will have it, that it ‘ passed over the Middle of the *Dodecatemorion* of *Aries*, which by ‘ Consequence would make it pass about Mid-way between the Rump ‘ and first of the Tail; ” which Situation could never be said to be over the Back: Whereas, if the Ring in this Cut was designed, as I apprehend, to image the *Colure* in the *antient Sphere*, it exactly answers *Hipparchus*'s Description— ἐν δὲ τῷ ἑτέρῳ κολούρῳ φησὶ κεῖσθαι τοῦ κριοῦ τὰ νῶτα κατὰ πλάτος. and justifies the Construction Sir *Isaac* put on those Words beyond Exception. The Sculptures from whence this was taken, have the Title of *Aratæa, sive Signa Cœlestia, in quibus Astronomicæ Speculationes Veterum ad Archetypa vetustissimi Aratæorum Cæsaris, Germanici Codicis* (44) *ob oculos ponuntur a Jacobo de Geyn ex Biblioth. Acad. Lugd. Bat. Amstel.* 1652.†

* Vol. VII. Part IV. § I. 2.

† *Hug. Grotii Batavi Syntagma Arateorum: ex offic. Plantin.* 4to. See *Germanicus*'s Interpretation, p. 35, the Figure of the Constellation *Aries*.

I will

I will beg Leave to obſerve farther, that as this Catalogue begins with the *Draco*, which the Ancients ſeem to place at the Head of their Conſtellations; perhaps it may give ſome Light into the Time of the Book of *Job*, as well as into the Senſe of that Place. For when he ſays, *By his Spirit he has garniſhed the Heavens*; *his Hand has formed the crooked Serpent*; I ſubmit it to the Judgment of the Critics, whether it is not highly probable the Writer muſt have lived within that Period of Time wherein a Star of that Conſtellation might paſs for the Polar Star: And then, if the Aſteriſms are ſuppoſed to be placed in ſome ſuch Order as here, the expreſs Mention that he only makes of this was ſufficient to refer us to the whole Syſtem or Furniture of the Heavens.

Chap xxvi. 13.

A Propoſal to make the Poles *of a* Globe *of the Heavens move in a Circle round the Poles of the* Ecliptic; *by the ſame.* No. 447. p. 201. April 1738.

XXXVII. 1. As we now have the Globes of the Heavens, they are only formed for the preſent Age, and do not ſerve the Purpoſes of Chronology and Hiſtory, as they might, if the Poles, whereon they turn, were contrived to move in a Circle round thoſe of the Ecliptic, according to the preſent Obliquity of this. By this Means we might have a View of the Heavens ſuited to every Period, and that would anſwer the ancient Deſcriptions, thoſe of *Eudoxus*, for Inſtance, who is ſuppoſed to borrow his from the moſt early Obſervations; and of *Hipparchus*, &c. Nor could any Contrivance better enable the loweſt Reader to judge of the Merits of the Controverſy about the *Argonautic* Expedition, as far as it depends on this: For it will verify to the Sight the Path of the Colures, &c. at any Time.

A Contrivance to make the Poles *of the Diurnal Motion in a* Celeſtial Globe *paſs round the Poles of the Ecliptic. Invented by* John Senex, *F. R. S.* Ibid. p. 203. May 1738.

2. The Poles of the Diurnal Motion do not enter into the Globe, but are affixed at one End, to two Shoulders or Arms of Braſs, at the Diſtance of 23° ½ from the Poles of the Ecliptic. Theſe Shoulders at the other End are ſtrongly faſtened on to an Iron Axis, which paſſeth through the Poles of the Ecliptic, and is made to move round, but with a very ſtiff Motion; ſo that when it is adjuſted to any Point of the Ecliptic, which you deſire the Equator may interſect, the Diurnal Motion of the Globe on it's Axis will not be able to diſturb it.

When it is to be adjuſted for any Time, paſt or to come, bring one of the braſen Shoulders under the Meridian, and holding it faſt to the Meridian with one Hand, turn the Globe ſo about with the other, that the Point of the Ecliptic, which you would have the Equator to interſect, may paſs under 0 Degrees of the braſen Meridian: Then holding a Pencil perpendicular to that Point, and turning the Globe about, it will deſcribe the Equator as it was poſited at that Time; and transferring the Pencil to 23° ½, and 66° ½ on the braſen Meridian, the Tropics and Polar Circles will be deſcribed for the ſame Time.

By this Contrivance, the Celeſtial Globe may be ſo adjuſted as to exhibit not only the Riſings and Settings of the Stars, in all Ages, and in all Latitudes, but the other *Phænomena* likewiſe, that depend upon the Motion of the Diurnal Axis round the Annual Axis.

Fig. 85. *a a a a.* A Section of the Celestial Globe.

E E. A strong Iron Axis, passing through the Poles of the Ecliptic.

b c. Two strong Arms of Brass, screwed on to the Ends of the Iron Axis, at *d.*

P P. The Axis or Poles of the Diurnal Motion, (by which the Globe is hung in the brasen Meridian) rivetted on to the other Ends of the brass Arms, and which may be carried round the Poles of the Ecliptic, by the Iron Axis, but with so stiff a Motion, as not to disturb the Diurnal Rotation on the Poles *P P.*

The true Delineation *of the* Asterisms *in the ancient Sphere By the Rev.*Ebenezar Latham,*M.D.* No. 460. p. 730. April *&c.* 1741.

XXXVIII. I never heard of Mr *Senex*'s Invention, till I saw the Transaction N° 447 *, and am pleased with the Opportunity I had of producing it to the World. It is many Years since I first thought of this Method, and have often suggested it to some Students. The Dispute that arose about Sir *Isaac Newton*'s Chronological Index, communicated by Abbé *Conti*, confirmed my Opinion of the Advantage that would attend it; especially the Admonition Dr *Halley* gave Father *Souciet*, (' to inform himself in the *Sphæriques*, so as to give ' us the right Ascension of the Stars truly from their given Latitude ' and Longitude') made me yet more sensible how necessary something of this kind was, to let common Readers into the Merits of the Controversy. But it was perfectly accidental, that I ever presumed to mention this Alteration in the Construction of the Globes, which I had so often wished might obtain for the Use of several Sciences. You will receive, with this, one Scheme, among several, which I have projected, that is nearest Mr *Senex*'s, and least defaces the Globe.

Fig. 86. Fig. 86. A Vertical Section of the Globe.

P. P. *The Poles of the diurnal Motion.*

A. *The Axle of the Globe, which terminates in the Poles of the Ecliptic, and receives the other End of the Brass Arms upon each of it's Pivots.*

Æ. *A brass Equator fixed to the brasen Meridian.*

K. K. *A Key, which, on Occasion, being put through a Hole in the Brasen Meridian, is just over the Place where the Poles of the Ecliptic pass, by means of a square Hole in the Head of a Screw, serves to fix that End of the Brass Arm, or give it Liberty to move with Ease: And the Key, being left in when the Screw is slackened, will hinder the Globe from moving on the Poles of it's diurnal Motion, till you have adjusted it to your Mind, straitened the Screw again, and taken out the Keys; as may be seen more plainly in,*

Fig. 87. Fig. 87. Which is nothing but the Windlass Part, or the Arm, Pole, and Part of the strong Axis of the Globe, with the Screw and Key more at large, and separate from one another for the more distinct View.

* *See* Sect. XXXVII. 2.

If I may take the Liberty to add any thing farther on this Head, next to the accurate Obſervation of the *Britiſh Catalogue* in placing the Stars themſelves, it ſhould be the Revival of the *ancient Figures* and *Colours*, as far as we can recover them. It is certain the Invention was very ancient, if we ſuppoſe the Deſcriptions *Eudoxus* has given us, taken from Obſervations long before his Time, when the Solſtitial Colure paſſed through the Middle of the *Great Bear*, and the *Crab*, through the Neck of *Hydra*, and cut *the Ship* between the Poop and the Maſt, *&c.*——Now I have mentioned *the Ship*, you will indulge a Conjecture, that the Situation of this [juſt on the Horizon (where they imagined the Sea) in an erect ſailing Poſture for ſome Eaſtern Expedition, and terminating their fartheſt View to the South,] may both give ſome Light into their *Latitude*, that impoſed this Name, and (from that, which muſt have been the Place of the Pole to anſwer this Form) *the Æra of Time*, wherein it was done; for, in the preſent Diſpoſition, the Inhabitants of *Greece* could not have a proper View of that Conſtellation, or be led to form it in the Manner the Ancients have done. I ſhall not here urge all the Difficulties in the old Deſcriptions, that might have a Solution from this Method; but if an Alteration could be made either in the *Colour* or *Attitude* of the *Figures* to anſwer them better, it would add to the Pleaſure of reading ſome Authors, and, together with that new Conſtruction, might afford us ſuch a View of the Heavens, as Mr *Addiſon* had of *Italy*, when he made the Tour of it with the Claſſics in his Hands: And, ſince I have brought thoſe Writings into the Account, you will allow me to cite ſome Paſſages, which might receive both *Truth* and *Beauty* from ſuch an Improvement: Where *Homer* ſays,

Πληϊάδας θ' Ὑάδας τε, τό τε σθένος Ὠρίωνος,
Ἄρκτον θ', ἣν καὶ ἄμαξαν ἐπίκλησιν καλέουσιν,
Ἥτ' αὐτοῦ στρέφεται, καὶ τ' Ὠρίωνα δοκεύει,
Οἴη δ' ἄμμορός ἐστι λοετρῶν ὠκεανοῖο.

Ἰλιάδ. Σ. 487.

The *Pleiads*, *Hyads*, with the Northern Team,
And great *Orion*'s more refulgent Beam;
To which around the Axle of the Sky,
The *Bear* revolving points *his* golden Eye,
Still ſhines exalted on th' ethereal Plain,
Nor bathes *his* blazing Forehead in the Main.

Mr Pope.

Mr *Pope*, amidſt a ſmall Miſtake of the *Sex*, keeps only the Forehead above Water; but the Poet ſeems to exempt her entirely; and ſo

does *Virgil*, when he makes *Fear* account for the ſame *Phænomenon*, that *Ovid* (who preſerves all the Fable of the Ancients) aſcribes to *Force.*

Maximus hic Flexu ſinuoſo elabitur Anguis
Circum, perque duas in morem fluminis Arctos:
Arctos Oceani metuentes Æquore tingi.
VIRGIL. Georg. Lib. I. 244.

Around our Pole the ſpiry *Dragon* glides,
And like a winding Stream the *Bears* divides,
The Leſs, and Greater, who by Fate's Decree
Abhor to dive beneath the* *Southern* Sea.
DRYDEN.

Nuper honoratas ſummo mea Vulnera Cœlo
Videritis Stellas illic, ubi Circulus Axem
Ultimus extremum Spatioque breviſſimus ambit.
OVID. Met. Lib. II. 515.

— — — — New Stars you'll ſee,
In this approaching Night's Obſcurity,
With hateful Beams i'th' *Arctic* Circle ſhine.

He immediately adds,

At vos ſi læſæ contemptus tangit Alumnæ,
† *Gurgite cœruleo ſeptem prohibete Triones:*
Sideraque in Cœlo ſtupri mercede recepta
Pellite, ne puro tingatur in Æquore Pellex.
——— 527.

Ne'er let thoſe ſpurious Stars approach the Deep,
Nor in the purging Ocean's Boſom ſleep,
But their eternal Stain, their whoriſh Tincture keep.

And when he deſcribes them as a Team, it is with the ſame Reſerve.

Tum primum Radiis gelidi caluere Triones,
Et vetito fruſtra tentârunt Æquore tingi.
——171.

Then the Sev'n Stars firſt felt *Apollo*'s Ray,
And wiſhed to dip in the forbidden Sea.

* Northern. † In the *Ordeal* by Water it was adjured, not to receive the Guilty, in Terms like theſe.

All which is a proper Hint for the *Difpofition of the Globe*, that muft correfpond to thefe *Appearances* then, and which can only be obtained by this Method: By the Help of which we may alfo apprehend the Light thefe Defcriptions give us *into the Age of the Writers*. I may illuftrate this from *Hefiod*'s Account of *the Seafons*, of which we have not only a better Idea by this artificial Difpofition of the Globe to anfwer them, but alfo of *the Time* wherein he lived, when we come to adjuft the Heavens to the accurate Inftructions he gives us, according to his Latitude at *Afcra*, allowing 50'' *per Annum* for the apparent Motion of the Stars.

Εὖτ' ἂν δ' ἑξήκοντα μετὰ τροπὰς ἠελίοιο
Χειμέρ' ἐκτελέσῃ Ζεὺς ἤματα, δή ῥα τότ' ἀστὴρ
'Αρκτοῦρος προλιπὼν ἱερὸν ῥόον ὠκεανοῖο,
Πρῶτον παμφαίνων, ἐπιτέλλεται ἀκροκνέφαιος.

Ἡσιόδ. Ἔργ. Βιβλ. β. 182

When the glad Sun, approaching with his Rays,
Has from the Tropic run out Sixty Days;
Arcturus, rifing from his facred Bed,
Is firft difcover'd in the Ev'ning Shade.

Εὖτ ἂν δ' Ὠρίων καὶ Σείριος ἐς μέσον ἔλθῃ
Οὔρανον, 'Αρκτοῦρον δ' ἐσίδῃ ῥοδοδάκτυλος Ἠώς.

——— 227.

But when *Orion*, and the *Dog-Star*, come
To the Mid-region of the heav'nly Dome,
The Morn, that blufhing draws away the Night,
Beholds *Arcturus* in the dawning Light.

If we fix the Pole almoft in the Mid-way between the Star in the Shoulder of the leffer *Bear*, and another of the *Serpent*, we fhall have the Satisfaction to obferve all thefe *Phænomena* anfwer the Defcription. I fhall not enter into the Calculation; for I would not anticipate the Pleafure, one, that hath no Notion of the Age of *Hefiod*, muft have, when he finds himfelf able, with fo much Eafe and Precifion, to determine it by thefe Characters *.

Hefiod's

* Since I wrote this, I had the Pleafure to find *Scaliger* concur with me———
Hefiodus florebat eo Sæculo, quo Arcturus ἀκρόνυχος oriebatur in Bæotia, viij Die Martii. Si quid hoc ad Conjecturam facit, faltem apud excellentes Aftrologos, qui ex hoc Parapegmate infra feptuaginta plus minus Annos Sæculum Hefiodi deprehendere poffunt. Animadverf. in Chron. Eufebii, *p.* 67. Edit. Lugd. Batav. 1606.———
The following Paffage in Sir *Ifaac Newton*'s Chron. *p.* 95. hath come to my Hands fince the former. ' *Hefiod* tells us, that, 60 Days after the Winter Solftice, the Star
' *Arcturus*

Hesiod's Account of the *Pleiads* is too particular not to demand our Attention, and require an Explanation in the same way †.

> Πληϊάδων Ἀτλαγενέων ἐπιτελλομενάων
> Ἄρχεσθ' ἀμητȣ̃, ἀρότοιο δὲ δυσσομενάων.
> Αἳ δή τοι νύκτας τε καὶ ἤματα τεσσαράκοντα
> Κεκρύφαται. Ἡσιόδ. Ἔργ. Βιβλ. β'. 1.

> Begin the Harvest, as the *Pleiads* rise.
> And take the Plough, when they withdraw the Skies;
> For Forty Days and Nights their glimm'ring Light,
> Obscur'd to us, no longer chears the Sight.

To this I might add *Homer*'s Image of the *Dog-Star*, but especially the exact Description in *Hesiod*.

> Ἀστέρ' ὀπωρινῷ ἐναλίγκιον, ὅς τε μάλιστα
> Λαμπρὸν παμφαίνῃσι λελȣμένος ὠκεανοῖο. Ἰλιάδ. Ε'. 5.

> Like the red Star, that fries th'autumnal Skies,
> When fresh he rears his radiant Orb to Sight;
> And, bath'd in Ocean, shoots a keener Light.

> ————δὴ γὰρ τότε Σείριος ἀστὴρ
> Βαιὸν ὑπὲρ κεφαλῆς κηριτρεφέων ἀνθρώπων
> Ἔρχεται ἠμάτιος, πλεῖον δέ τε νυκτὸς ἐπαυρεῖ.
> Ἡσιόδ. Ἔργ. Βιβλ. β'. 35.

> For then the *Dog-Star* governs in his Course,
> Walks o'er the Heads of Men, who feel his Force,
> Comes in the Day, but chiefly shares the Night.

How beautifully does the same Writer express the Gesture of *Orion*, as he is following the *Pleiads*?

' *Arcturus* rose just at Sun-set; and thence it follows, that *Hesiod* flourished about 100 ' Years after the Death of *Solomon*, or in the Generation or Age next after the *Trojan* War, ' as *Hesiod* himself declares.'

'Tis what we may compute by the present Globe; for, bringing *Arcturus* to the Eastern Horizon, the Sun we shall find in the Ninth Degree of *Aries*. Now it enters ♑ *Dec.* 11. and 60 Days after, or *Feb.* 10. it is in ♓ 2° 30′ when allowing for the Northern Latitude of *Arcturus* to make it visible on the Horizon, it's Longitude must have been ♍ 14°, *&c.* whereas it's Place now is about ♎ 20° 27′ 12″. And the Difference both ways one Sign 6° 18′ *&c.* which makes him to have lived 2614 Years ago.

† *Hisce Signis veteres Agricolæ, & ex eorum Traditionibus Scriptores rei rusticæ, nec non & Medici, Poëtæ, & Historici sunt usi ad Anni Tempestates designandas,* &c. Greg Astron. *p.* 130.

Εὖτ' ἂν Πληϊάδες σθένος ὄβριμον Ὠρίωνος
Φεύγουσαι, πίπτωσιν ἐς ἠεροειδέα πόντον. ———237.

The *Pleiads*, flying from the threat'ning Scourge
Of ſtrong *Orion*, plunge into the Surge.

Perhaps this may give ſome Light to a Paſſage of *Virgil*, that hath very much puzzled his Commentators.

Taygete ſimul Os terris oſtendit honeſtum
Pleias, *& Oceani ſpretos pede reppulit Amnes :*
Aut eadem Sidus fugiens, *ubi Piſcis aquoſi*
Triſtior hybernas Cœlo deſcendit in Undas.
Georg. Lib. iv. 232.

Firſt, when the pleaſing *Pleiades* appear,
And ſpringing upward ſpurn the briny Seas :
Again when their affrighted Choir ſurveys
The watry *Scorpion* mend his Pace behind,
With a black Train of Storms, and Winter-wind,
They plunge into the Deep, and ſafe Protection find.
DRYDEN.

Some, I know, by this *Sidus* underſtand the *Southern-Fiſh*, others the *Hydra*, and ſome the *Sun* ; but how Mr *Dryden* came to inſert *Scorpio*, I ſhall not inquire. Nor ſhall I trouble you with any Conjectures with regard to the *ancient Figures :* It is certain there have been *Variations* in this reſpect, ſince *Ptolemy* mentions a Star in the Horn of *Aries* ; and it is thought *Hipparchus* reckoned one, that is now in the Line, to the firſt Foot of *Aries* *. Whether the Epithet *Ovid* gives *Capella*, does not imply ſome little Difference, in the Situation of it, from ours, I leave to the Critics.

——*Et* Oleniæ *Sidus pluviale Capellæ,*
Taygetenque, Hyadaſque Oculis, Arctonque notavi.
Met. Lib. III. 594.

* Since I wrote this, I find Sir *Iſaac Newton*, in this way, recover to their former Places the Stars below, by rectifying the Delineation.
' In the extreme Fluxure of *Eridanus*, a Star of the Fourth Magnitude, of late ' referred to the Boſom of *Cetus*.
' In the Head of *Perſeus*, a Star of the Fourth Magnitude.
' In the Right Hand of *Perſeus*, a Star of the Fourth Magnitude.
' In the Neck of *Hydrus*, a Star of the Fourth Magnitude.
' In the Left Hand of *Cepheus*, one of the Fifth Magnitude.'
All whoſe Characters he deſigns from *Bayer*.

—I began

——I began to note
The ſtormy *Hyades*, the rainy * *Goat*,
The bright *Taygite*, and the ſhining *Bears*,
With all the Sailors Catalogue of Stars.

I might inſiſt on the Etymology of *Arcturus*, and others; for it appears from the Accounts the Ancients themſelves give us, there was not always the greateſt Uniformity in their Drawings. *Ovid* ſays of *Bootes*.

— — — — — *& te tua Plauſtra tenebant.*
Lib. 2. 177.

Nay, and 'tis ſaid, *Bootes*, too, that fain
Thou would'ſt have fled, though cumber'd with thy Wain.
ADDISON.

And he lets us know, that *Scorpio* took up 60°.

Eſt Locus, in geminos ubi Brachia concavat Arcus
Scorpios; *& Cauda, flexiſque utrinque Lacertis,*
Porrigit in Spatium Signorum Membra duorum.
——195.

There is a Place above, where *Scorpio*, bent
In Tail, and Arms, ſurrounds a vaſt Extent;
In a wide Circuit of the Heav'ns he ſhines,
And fills the Space of Two celeſtial Signs.

This might be one Reaſon of that Compliment which *Virgil* paid *Auguſtus*, apart from the other, which *Scaliger* aſſigns.—

Anne novum tardis ſidus te menſibus addas,
Qua locus Erigonem inter, Chelaſque ſequentes
Panditur? ipſe tibi jam Brachia contrahit ardens
Scorpius, *& Cœli juſta plus parte reliquit.*
Georg. Lib. I. 32.

Where in the Void of Heav'n a Space is free,
Betwixt the *Scorpion*, and the *Maid*, for thee:
The *Scorpion*, ready to receive thy Laws,
Yield half his Region, and contracts his Claws.

'Tis true, this Poet knew *Libra* very well; but, perhaps, it made no great Figure among the *Aſteriſms* then.

* Elbow'd.

Libra

Libra *die ſomnique pares ubi fecerit horas,*
Et medium Luci, atque Umbris jam dividit Orbem.

—208.

But when *Aſtræa*'s Balance, hung on high,
Betwixt the Nights and Days divides the Sky.

DRYDEN.

How *Taurus* was *painted* at that Time, we learn from his Deſcription.

* Candidus auratis *aperit cum* Cornibus *Annum*
Taurus, & averſo *cedens Canis occidit Aſtro.*

—217.

When with his golden Horns, in full Career,
The *Bull* beats down the Barriers of the Year;
And *Argos*, and the *Dog*, forſake the Northern Sphere.

In the laſt Verſe we have, perhaps, no Occaſion for *Heinſius*'s Correction of *adverſo*, if we compare the Diction here with *Ovid*'s.

Per tamen adverſi gradieris Cornua Tauri.
Met. Lib. II. 80.

The *Bull*'s oppoſing Horns obſtruct the Way.

The Inſtructions *Virgil* gives in the ſame Place, as to Huſbandry, are beſt underſtood from this new Diſpoſition, and may render us ſenſible how much earlier theſe *Phænomena* were then in the Year, to what they are at preſent †.

Ante tibi Eoæ Atlantides abſcondantur, &c.
Georg. Lib. I. 221.

But if your Care to Wheat alone extend,
Let *Maia* with her Siſters firſt deſcend,
And the bright *Gnoſian* Diadem downward bend.

* By reaſon the Firſt Month of the old Luni-ſolar Year (on account of the intercalary Month) began ſometimes a Fortnight after the Æquinox. This may, perhaps, account better for the Propriety of *Virgil*'s Expreſſion *Aperit Annum*, than any of his Commentators have done.

† *Paulatim Obſervatio hujus Ortûs & Occaſus neglecta jacet, nec ab aliis uſurpatur, quàm à Poëtis, qui tempora per Circumſtantias tam varii Ortus & Occaſus tot Syderum (quibus nihil pulchrius) deſcribere, & veluti pingere ſolent, quamvis plerumque erroneè, quippe qui Calendarii noſtri Diem per ejuſdem Stellæ Ortum deſcribunt nunc, per quem deſcribebatur tempore Cæſaris, cùm tamen tempora diſcrepent* 14 *diebus ferè.* Greg. Aſtron. *p.* 132.

I know we cannot depend upon all the Exactneſs in a Poet, that might be expected from an Aſtronomer: But *Virgil* ſeems to have made it his favourite Study.

Me vero primum dulces ante omnia Muſæ,
Quarum ſacra fero ingenti perculſus Amore,
Accipiant; Cœlique Vias, & Sydera monſtrent.
Lib. II. 475.

Would you your Poet's firſt Petition hear,
Give me the Ways of wand'ring Stars to know.

Ovid appears alſo perfectly acquainted with the ancient Figures, and the moſt accurate way of *delineating* them, at the ſame time that he enlivens them with their *Fictions*.

Conſiſtuntque Loco, Specie remanente Coronæ,
Qui medius nixique Genu, anguemque tenentis.
Met. VIII. 181.

— — — — — The Crown retains
It's proper Figure, and a Station gains
Where *Hercules* in bending Poſture ſtands,
And ſtrives to gripe the *Dragon* in his Hands.
Vid. Lib. XIV. 846.

How we came by the Account, it is not material to inquire; but there is *one Line*, wherein he ſeems to have preſerved ſome ancient Tradition, as to the *Pole*.

Quæque Polo *poſita eſt glaciali* proxima *Serpens.*
Lib. II. 173.

The folded *Serpent* next the frozen Pole.

And there is Reaſon to believe one of the Stars of that *Conſtellation* was the *ancient Polar Star*, and might firſt give Riſe to the Denomination; for one in the *Tail of the Dragon*, of *the Third Magnitude*, comes neareſt it of any other. About the Time of *the Flood*, it was within 10′ *of the Pole*, and might paſs for the *Polar Star* a Thouſand Years after among thoſe Writers, from whom *Ovid* copied his Expreſſion. However, this is certain, that *another Star* of that Conſtellation, one of the Fourth Magnitude, was really nearer than any other, when the old *Obſervations* were made, which literally juſtifies *Ovid*'s Account. I might take notice of his exact Repreſentation of the Diſpoſition of *the Ara*, and *Anguis*, when he makes them the two Extremes.

—*Medio*

Fig. 83.

Fig. 84.

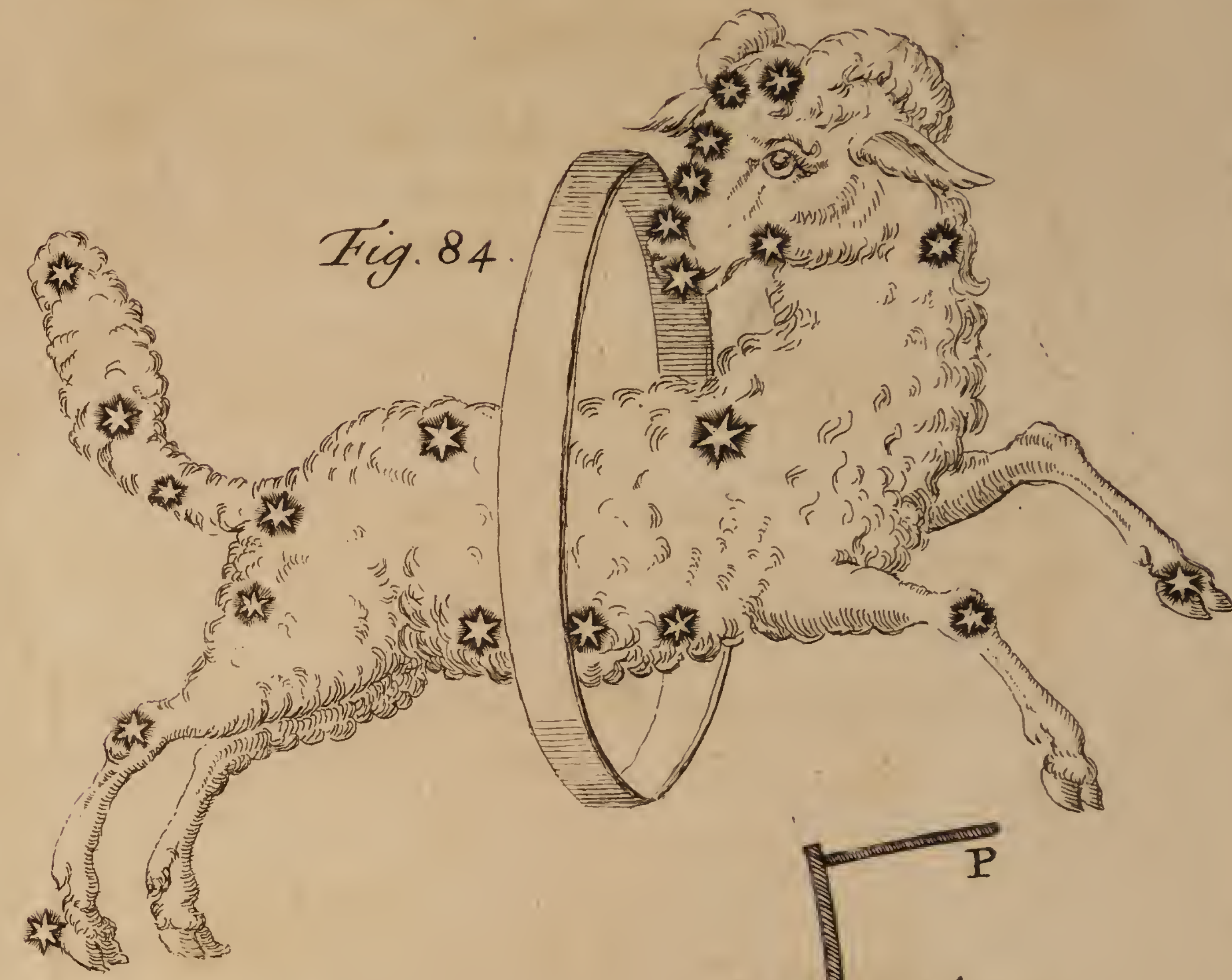


Fig. 85.

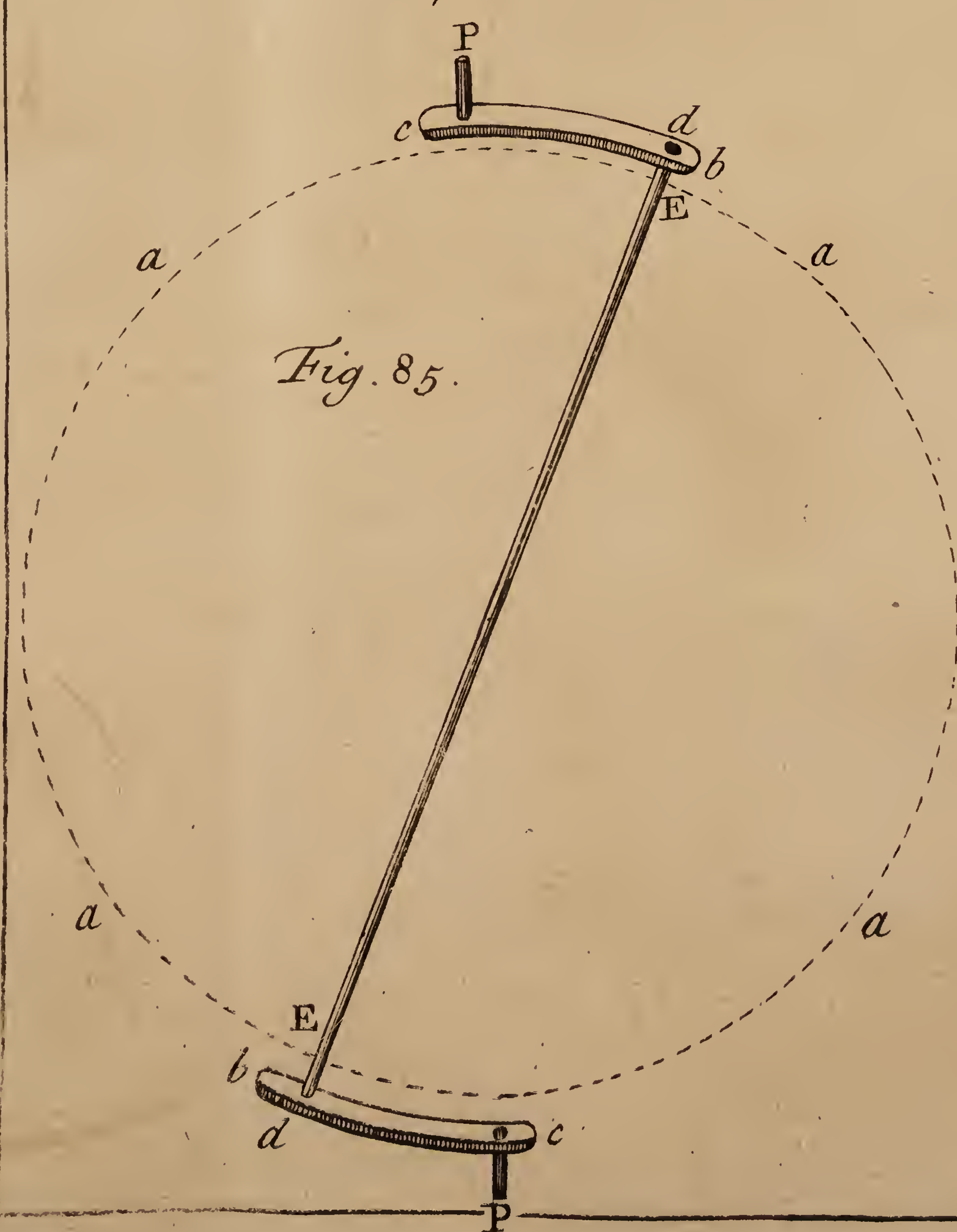


Fig. 86.

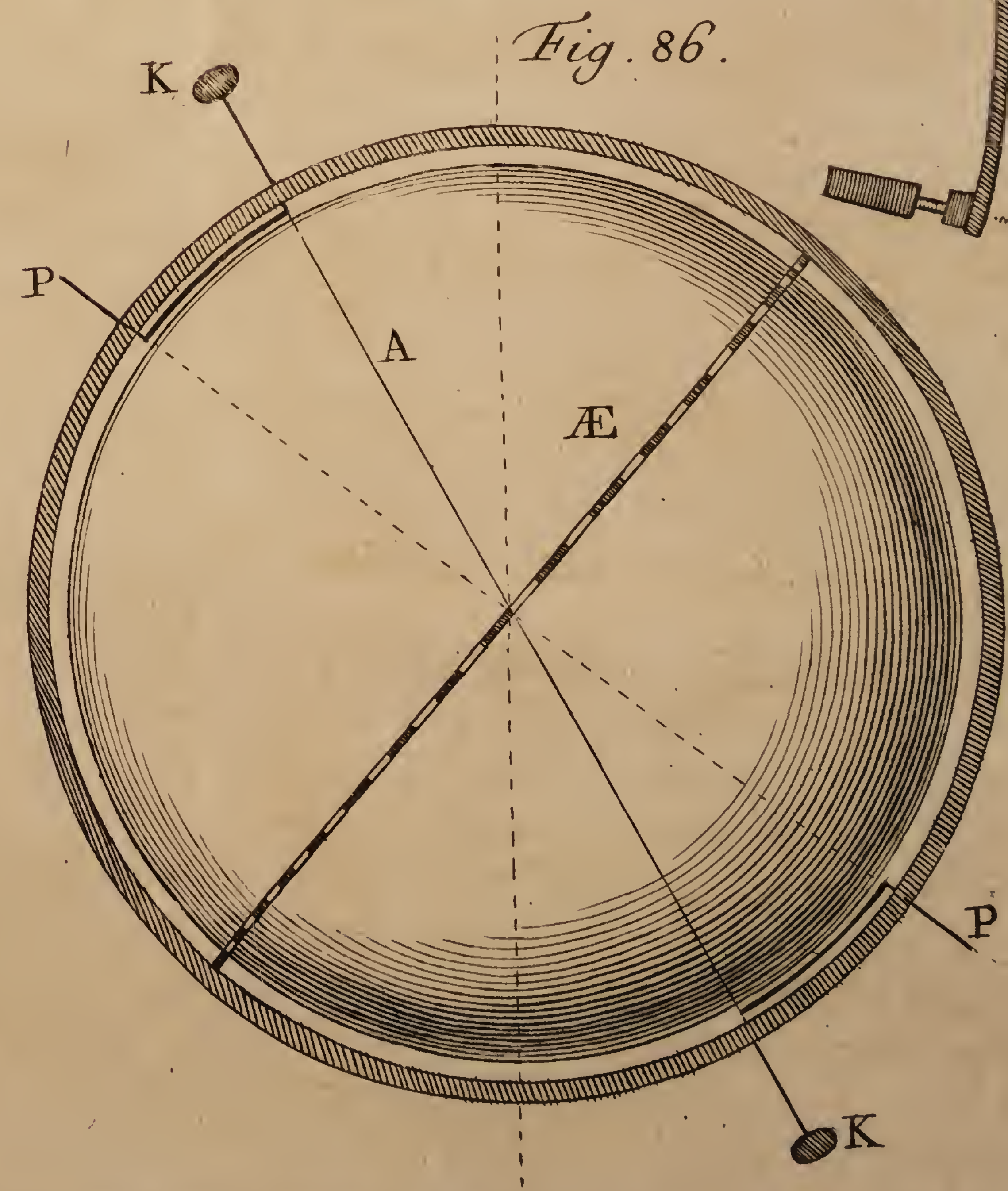


Fig. 87.

— — — — *Medio tutissimus ibis:*
Neu te dexterior tortum declinet in Anguem,
Neve sinisterior pressam Rota ducat ad Aram.
Inter utrumque tene.——— ib. 137.

— — — — The middle Way is best,
Nor where in radiant Folds the *Serpent* twines
Direct your Course, nor where the Altar shines.
Shun both Extremes.

But the Inspection of the Globe, when it is fixed in a proper Position, will convey the best Idea of all these Appearances; for we derive this Advantage from the new Construction of it, that it will enable us to place the several *Phænomena before every Eye*; by which means those who have the least Acquaintance with these Studies, must be greatly surprized, and pleased to observe the ancient Accounts minutely verified. It is a sort of living over again the former Ages, *allowing* 1°. 23′. 30″. *for every hundred Years*, according to *Ricciolus* and *Flamsted*, which is a sort of Mean between the other Computations.

I shall not now suggest some other Purposes, that might be served by this Method. It is sufficient to recommend the Invention, that it throws so much Light on the common Classics, to which I have confined this Examination, and which must be my Excuse for the Citations.

PAPERS omitted.

1. A Catalogue of Eclipses of *Jupiter*'s Satellites, for the Year 1734, by *James Hodgson*, F. R. S. Master of the Royal Mathematical School at *Christ's Hospital London*. No. 427. p. 26.

2. The same for the Year 1735. No. 432. p. 279.

3. The same for the Year 1736, computed to the Meridian of the Royal Observatory at *Greenwich*, by the same. No. 436. p. 5.

4. The apparent Times of such of the Immersions, and Emersions of *Jupiter*'s Satellites, as are visible at *London*, in the Year 1736, together with their Configurations at those Times, represented in a Plate. Ibid. p. 13.

5. An Account of some Observations of the Eclipses of the first Satellite of Jupiter, compared with the Tables, by the same. Ibid. p. 15.

6. The apparent Times of the Immersions and Emersions of *Jupiter*'s Satellites, which will happen in the Year 1737, computed to the Meridian of the Royal Observatory at *Greenwich*, by the same. No. 440. p. 177.

7. The apparent Times of such of the Immersions and Emersions of *Jupiter*'s Satellites, as are visible at *London*, in the Year 1737, by the same. Ibid. p. 184.

No. 443. p. 301. 8. The Immerſions and Emerſions of the four Satellites of *Jupiter*, for the Year 1738, computed to the Meridian of the Royal Obſervatory at *Greenwich* by the ſame.

Ibid. p. 309. 9. The apparent Times of ſuch of the Immerſions and Emerſions of *Jupiter*'s Satellites, as are viſible at *London* in the Year 1738, by the ſame.

No. 445. p. 69. 10. The apparent Times of the Immerſions and Emerſions of *Jupiter*'s Satellites for the Year 1739, computed to the Meridian of the Royal Obſervatory at *Greenwich*, by the ſame.

Ibid. p. 76. 11. The apparent Times of ſuch of the Immerſions and Emerſions of *Jupiter*'s Satellites, as are viſible at *London*, in the Year 1739, by the ſame.

No. 449. p. 332. 12. The apparent Times of the Immerſions and Emerſions of the four Satellites of *Jupiter*, for the Year 1740, computed to the Meridian of the Royal Obſervatory at *Greenwich*, by the ſame.

Ibid. p. 340. 13. The apparent Times of ſuch of the Immerſions and Emerſions of *Jupiter*'s Satellites, as are viſible at *London*, in the Year 1740, by the ſame.

No. 471. p. 602. 14. De Diſparitione Annuli Saturni An. 1743, & 1744, ex Epiſtola a Domino Godofredo Heinſio, ad Dominum Petrum Colinſonum, R. S. S. data.

CHAP. IV.

Of SURVEYING.

A new Plotting Table *for taking* Plans *and* Maps *in* Surveying: *Invented in the Year* 1721, *by* Henry Beighton, F. R. S. No. 461. p. 747. Aug. &c. 1741. Fig. 88. Fig. 89.

IT is a plain ſmooth Board, about 18 Inches ſquare, and Three-quarters of an Inch thick, Fig. 88. *A B C D*, made of *Mahogany*, *Walnut*, *Pear-tree*, or *Norway Oak*, well clamped at the Ends, or a braſs Frame round it, to prevent it's warping, and, as much as poſſible, ſhrinking and ſwelling.

Within Six-tenths of an Inch of two of it's oppoſite Sides (and parallel to them and one another) are two Grooves *E F*, *G H*, cut on the Face half an Inch deep, to let in two braſs Holders in the Shape of *N O*, Fig. 89. which are each of one Piece of caſt Braſs, like two braſs Rulers, joined together at Right Angles. The perpendicular Part is $\frac{1}{10}$ and $\frac{3}{100}$ Parts of an Inch thick, as at *d*, $\frac{1}{2}$ an Inch deep, and a little ſhorter at each End than the upper Part, which is 17 Inches long, $\frac{3}{10}$ broad, and about 8 Parts of $\frac{1}{100}$ of an Inch thick; about $2\frac{1}{2}$ Inches from each End of the Holder, are thick Parts of Boſſes in the upright Piece, as at *P* and *Q*, through which are Holes tapped, to receive the Screws *P S*, *Q R*, which Screws go each through a braſs Plate as *T* and *V*, fixed by Rivets on the under Side of the Table, and little round Nuts, (as at *a* and *b*) put on them,

to confine them to their Shoulders in turning in the Plates, that they neither rise nor fall; these Holders must go easy in the Grooves, to sink easy and even with the upper Surface of the Table. Then, when the Screws enter the Holes of the Holders, by turning *R* and *S* at the same Time forward, the Holders will fall, and pinch down any Papers, &c. that are under them; and, turning backward, will rise and release them. In the Middle of one End of the Table is a Groove to receive the Brass *W*, which has the same sort of Screw and Fixing as the other, to raise or fall it. But the Groove is quadrantal, that the Holder *W* may on Occasion be turned so as to lie all on the outside the Line *E H*, and to cross it, in case of high Winds, for securing the Papers down, on Three Sides; and a Fourth might be added, but there is seldom any Occasion for it.

To the Centre of the Table underneath, is fixed a brass Socket, so truly made, that the Table may, when set, turn round truly horizontally: And a Machine, cased with Glass, in which a Plummet, hangs to set the Table level; or the parallel Plates, and glass Tubes of Spirit of Wine, may be used, to set it horizontal, as any one sees Occasion to fansy them.

To any one of the four Edges underneath, is screwed a Box and Needle, set to the Variation.

There belongs to this Instrument, a strong three-legged Staff, and an Index with plain or telescopical Sights, near two Feet long.

The Papers, or Charts, for this Table, are to be either a thin fine Pastboard, fine Paper pasted on Cartridge-paper, or two Papers pasted together; cut as exactly square as is possible, each Side being nearly 16 Inches and an half long, just as they may slide in easily between the upright Part and under the flat Part of the Holders.

Any one of these Charts will be put in the Table any of the four ways, be fixed, taken out, and changed at Pleasure: Any two of them may be joined together true on the Table, if you make each of them meet exact at the Line *L M*, whilst near one half of each will hang over the Sides of the Table; or by cresting and doubling each, the whole of them still be within the Table. And if Occasion should happen, as seldom it does, by cresting each Paper both Ways through the Middle, four of them may be put on at one time, meeting in the Centre of the Table.

Each Chart is always crossed by Right Angles through the Middle, for the Purpose above, and to make any of them answer to the Guide-Lines on the Table, Fig. 88. *I K*, *L M*, drawn quite through the Centre, and the whole Table.——So the grand Objection of shifting Papers is obviated. Fig. 88.

It's Facility and Dispatch,

As also it's Certainty, compared with any of the most celebrated Instruments, I shall now briefly set forth.

But,

But, in order thereto, it may not be improper to premiſe, or lay down, as *Lemmata*, theſe three Things:

1. *The eſſential Buſineſs or Aim in ſurveying of Lands or Countries, is either to have an exact Plan, or to find the* Area *in ſome known Meaſure.*
2. *Every thing that is ſuperfluous or foreign to ſuch Deſign, is better omitted than taken.*
3. *If a true Survey, and exact Plan be made, every Part will have it's juſt Proportion, and every Angle it's true Opening or Quantity.*

Then what need have we of Degrees, Minutes, *&c.*? They are never made any Uſe of in the Practice of caſting up, or any thing related thereto: For, if from a Station two Lines be drawn by a good Index to two diſtant Objects, will it not be the very Angle, and identically the ſame, as if it had been taken by the moſt celebrated Inſtrument, in Degrees and Minutes, and laid down by a Protractor?

The firſt is much more expeditious, eaſy, and certain, than the other. More expeditious, becauſe thoſe two Lines are ſooner drawn than an Angle can be taken, which done, two thirds of the Work is behind, *viz.* Writing down and Plotting. More eaſy, as done with $\frac{1}{4}$ of the Trouble. More certain, becauſe one may be liable to Miſtakes in taking the Degrees or Minutes; in ſetting down, and in protracting. And if it ſhould ſo happen, that one numerical Angle ſhould be taken, ſet down, or plotted to the wrong Coaſt, (where they depend on one another) ſo great an Error would enſue, that could not be retrievable, but by going on the Spot, and performing the Operation anew. Now the Plotting-Table, after two Stations, proves every thing on the Spot; for, from every Station you are upon, the Index muſt point at the ſame time to any Station on your Map, and it's correſponding Object in the Field; which is a demonſtrative Proof, for nothing but Truth will agree.

In ſeveral Branches of the Mathematics, it is abſolutely neceſſary to take Angles in Degrees, Minutes, and their Subdiviſions, as Aſtronomy, Trigonometry, Navigation, Longimetry, inacceſſible Heights and Diſtances, *&c.* and alſo in taking large Plans, to calculate and prove Things by Trigonometry; which would not only be a Work of Curioſity, but very commendable. But where the Nature of the Thing will admit of as good Proof, with $\frac{1}{10}$ part of the Trouble and Time; it would be like running the Solution of an eaſy Queſtion into a long Proceſs of Algebra or Fluxions, when the plain Rule of Proportion would juſtly anſwer the ſame.

It is objected, That, in ſurveying by the Plotting-Table, the ſhrinking or ſwelling of the Papers, are a great Inconveniency.

In

In Anſwer to this, it may be ſaid, The ſame Inconveniency attends the ſurveying by any other Inſtrument, ſo ſoon as it is plotted; for both Velum and Paper will ſhrink and ſwell in the Houſe on the Alteration of Weather (as well as all Bodies); for a Line of 48 Chains, plotted by a Scale of 3.2 *per* Inch, in a hazy Morning, in a clear Afternoon the ſame Day, meaſured but 47 and an half: And there are various Shrinkings and Swellings, according to the Weather, and Difference of Paper, *&c.*

In the Plotting-Table this Inconveniency is in a great meaſure remedied. For in what State ſoever of the Weather you put Lines on the Chart, the Holders give Marks on the Chart as it then ſtood; if it was moiſt and ſwelled up in the middle Part, you may, when you either caſt up or meaſure Lines, by laying it on a damp Floor, put it in the ſame Condition as it was when you plotted the Lines. If you plotted in dry hot Weather, and are caſting up in damp or moiſt, a little heating by the Fire will reduce it to the ſame State again. Another Remedy I have long uſed is, to plot and meaſure by Scales of the ſame Paper, which will ſhrink or ſwell in proportion as your Map does.

But it will be well to obſerve here, that the ſhrinking and ſwelling alters the Lines only, and not at all the Angles: For, let a Polygon be never ſo much uniformly extended or contracted, each Angle muſt contain the ſame Number of Degrees and Minutes as before. Hence this Objection falls no harder on the Table, than on all other Inſtruments.

And here I intended to have ended this Diſcourſe: But as I have ſome other ſmall Improvements, not only in the Inſtrumental Part, but in a new Method of diſpoſing the Maps, and better adapting them to all ſubſervient Uſes; I proceed.

I ſhould have ſaid before, that each Chart has a *Flower de Lys* on it's North Edge; and, as the Needle is moveable to any Side, Care muſt be taken, that the North End of the Needle, when it ſtands, ſhould point the ſame Way as the *Flower de Lys* on the Charts.

I uſe a Needle about 5 Inches long, placed in an oblong wooden Box, but juſt ſo wide as the Needle may play double the Degrees of the Variation Weſt, *viz.* 30°. In the Middle of one End is the *Flower de Lys*, and the Box is by Studs and Holes always put on the Table oblique to the Quantity of the Magnetical Variation. I make no other Uſe of the Needle, than to ſet the Table in the Meridian, and to prevent any great Miſtakes, in joining or placing the Charts wrong.

I have no more than ½ an Inch of the Needle that appears from under the Table, for the Reaſon it ſhould not be in the Way, or ſo ſubject to be damaged: The making the Box ſo narrow, is to check it's playing, that it may ſooner hang ſtill over the *Flower de Lys.* The wooden Box, lined with Paper, I find preferable to a large braſs Box, and large Glaſs, which in cold and hazy Weather, condenſes the Vapour and Air ſo much, as to make the Needle very languid and dull.

The

Farther Uses, by taking a Survey in the new Method by the Plotting-Table.

The Charts, thus taken, are more readily laid together by Numbers on their Edges, which tally, and make up the whole Map in one Plan, or View, and are, in these Squares, more portable.

In the second Place, they are more readily copied, extended, or contracted. For, by having a Frame of Wood that just encompasses a Chart, divided by 19 Threads at equal Distances, and the same at Right Angles, the other Way; each Five or Ten, &c. being distinguished by Silk of a different Colour; a Reet is made of 400 Geometrical Squares, from which, having a Velum or Paper so divided by lesser or greater Squares; then drawing or copying by Help of the Lines into those new Squares, you have your true Map contracted or extended.

Large Maps of Lordships are not any ways convenient, or portable, to have recourse to on the Spot or Place they represent; being subject to Damages, unfit to be opened in rainy Weather, very troublesome in the Wind, and very difficult to find out the Part you want. To remedy all these Inconveniences, some Years ago I contrived a new Method of disposing them, in such Manner as makes them more sure, safe, ready, convenient, durable, and portable, than any other Method.

And this is done by imitating the Geography of the World, which first gives the whole, then the several Kingdoms, Countries, Provinces, and minuter Parts and Divisions, severally and more at large.

First, It will be highly necessary, that a *General Map* of the whole Lordship (Country, &c.) be drawn in one Sheet of Paper or Velum, to give the Form, Idea, and Proportion, that the Parts bear to the whole, and one another; by which Situations, Bearings of the Towns, Villages, Roads, and remarkable Places, will be seen at one View: And this must be reduced to so small a Scale, as the intended Sheet may comprehend the whole. A Scale of about 11 or 12 Chains in an Inch, will plot a Lordship of more than 2000 Acres, in the Compass of 16 ½ Inches square; which may be a convenient Size to make two Leaves, and open in a Folio Book. This Map may express the Roads, Rivers, Streets, Boundaries, Inclosures, and common Field Lands singly, in case they be not less than 40 or 50 Links in Breadth: The Pieces that contain not less than about 10 Acres, will admit of Room to write the Owners Names and Quantities in Statute Measure, as in

Fig. 91. Fig. 91 But for all the small Parts, there will not be room to explain them: Therefore I divide the general Map into as many Geometrical Squares, as it took Charts in surveying by the Table, by red Lines,

Fig. 90. as in Fig. 90 horizontally and perpendicularly, as noted by 0,0,0,0, &c. which, by a Scale of 32 *per* Inch, may take about 15 Charts in Number: In the openest Place near the Middle of each Square, in a small Circle, I number them with red Figures 1, 2, 3, &c. corresponding to the original Charts: And in the Middle of each of their Sides, Numerical Letters, shewing how the particular Maps are to join to each other.

The

The particular Maps are each as large as the general, and numbered at the Top I. II. III. *&c.* correſponding to the Squares in general, as Fig. 91. where, in the Right Hand Margin, is put V, and at the Bottom IX, ſhewing the Fifth Map tallies to the Side, and the Ninth to the Bottom, or South Part: The general Map being an Index, ſhewiug how they join to each other. Fig. 91.

By theſe particular Maps may be ſhewn all the leſſer Quantities, with their Tenure, Owners Names, and Contents; and, by the Scale, are capable of ſhewing the Lengths of any Lines, and the Dimenſions, ſo as to diſcover any Encroachments, and record their Shape and Extents to Poſterity: A moſt valuable Uſe of a Survey and Map.

All theſe Maps are bound up in Order, in a Folio Book, to open freely, which will be not only very portable, but uſeful to have recourſe to on any Occaſion; ſecure from Damages of Weather, as well as more durable and ornamental.

The Terriers to theſe Maps are made in the following Manner; either bound in a Book of a Pocket Size by themſelves, or along with the Maps.

The Names of the Freeholders, Copyholders, Cottagers, Tenants, *&c.* are put in an Alphabetical Order.

Tho. Power.

Refer to the Map.	The Names of the Lands, their Situations, and Boundaries.	Tenure	Freehold			Copyhold			
			a	*r*	*p*				
IV. f. 4.	*Calmer-Cloſe* in the Village of *B.*———the Pariſh of *Gwin W. Townſend* E. Own S.	Freehold	11	1	—	—	—	—	
IV. f. 6.	The Houſe and Home-ſtead called *Broadmoor Horſe Cloſe* W. Own E. N. S.	Copyhold	—	—	—	18	2	28	
IV. e. 6.	*Horſe Cloſe Guinne* W. *Broadmoor* E. *Pitts* N S.	Freehold	17	1	6	—	—	—	

In like Manner, under every different Name, may all the Parcels be expreſſed ſeparately.

To find any Piece or Parcel of Land in the Lordſhip readily, firſt find the Tenant's or Owner's Name in the Alphabetical Order, under which, in the Second Column, may the Parcel be found. The 3d ſhews whether it is Free or Copyhold; the 4th or 5th, the Quantity in Statute Meaſure, either Free or Copyhold.

The numerical Letter in the Margin on the Left IV. ſhews it is in the Fourth particular Map; *f.* 6. refers to the Parts of the Map; find *f.* at the Top, and 6 on the Left Side, and in the Angle of Meeting of thoſe Squares is the Houſe, Clоſe; and ſo for any other.

There is but one Objection I can at preſent foreſee, that can bear any Weight againſt this Method of dividing the general Map, *viz.*

 That

That by dividing the ſame into geometrical Squares, many of the Parcels, Lands, and Grounds, will be cut into two ſeparate Pieces; one Part whereof will lie in one particular Map, and the reſt in another; as in Fig. 91. Map IV. Part of *Calmer* and *Broad-Cloſe* will be in the Vth Map.

In this Caſe, it is uſual to put the Owner's Name, and Quantity, in that which is the greater Part, and in the Terrier refer alſo to the Remainder; where, if the Shape, Lengths, *&c.* are required, they may be diſcovered.

But as this may not be ſatisfactory, or fully anſwer the Objection; the two following Methods will entirely obviate the Difficulty, and make them as fully ſubſervient to all Purpoſes, as any large and entire Map on one Piece.

The 1ſt Method is, to take juſt ſo much in a particular Map as is circumſcribed by ſome known Roads, Lanes, Brooks, Boundaries of particular Owners, or Tenants Lands: This, indeed, will often make the Map very diſproportional, and irregularly ſhaped; but cannot be a material Objection, by reaſon, in Surveys, there is ſeldom any thing regularly ſhaped.

2. The 2d Method is, to have a wider Margin, or rather draw the particular Maps by a ſmaller Scale, as 4 Chains in an Inch, inſtead of 3 Chains 20 Lines; and that will allow Room to add the Parts of the Fig. 92. Parcels ſo cut off in the Margin, as in Fig. 92. the IVth particular Map varied, where the Whole of *Broadmoor* and *Calmer* is drawn; then in the Vth and IXth particular Map, may the ſmall Parts, which are in the IVth, be drawn in full: Then will they join by indenting or tallying one into anotherr

To reduce a Scale to fit exactly your general Map.

Firſt ſee what Extent the whole Survey takes on the Charts you laid it down by in the Field, *viz.* the greateſt Depth and Breadth, as from the Specimen of the general Map it may appear.

	Depth		Breadth		
On the upper Chart is N°	2. =	10 Inches.	N°	8 =	5 Inches
	6.	$16\frac{1}{4}\frac{1}{2}$		9	$16\frac{1}{4}\frac{1}{2}$
	11.	$16\frac{1}{4}\frac{1}{2}$		10	$16\frac{1}{4}\frac{1}{2}$
	13.	$10\frac{1}{2}$		11	$16\frac{1}{4}\frac{1}{2}$
				7	$6\frac{1}{2}$
The Whole		$53\frac{1}{4}$	The Whole		$60\frac{1}{2}\frac{1}{2}$ qr.

Then having fixed on the Size of the general Map to be 16,37 Square, I form a Scale of 60 $\frac{1}{2}$ *per* Inch, that may juſt extend the whole Breadth of the 16,37 Inches; by which you may form all the Squares, and Parts of Squares, in Depth and Length, as above; and at Fig. 90. is divided.

The

Fig. 88.

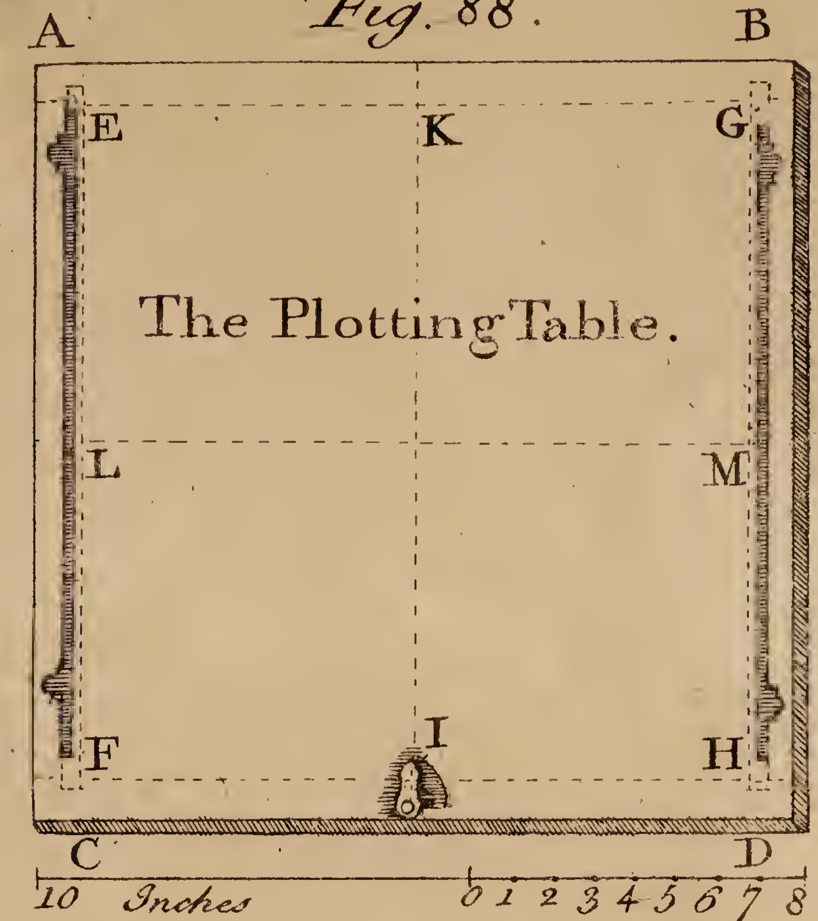


Fig. 89.

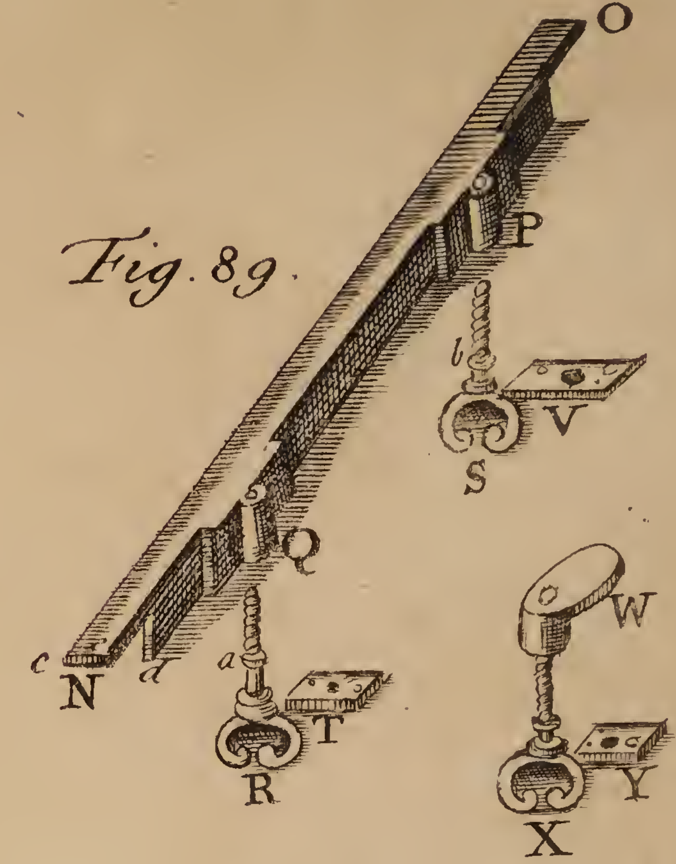


Fig. 90. General Map

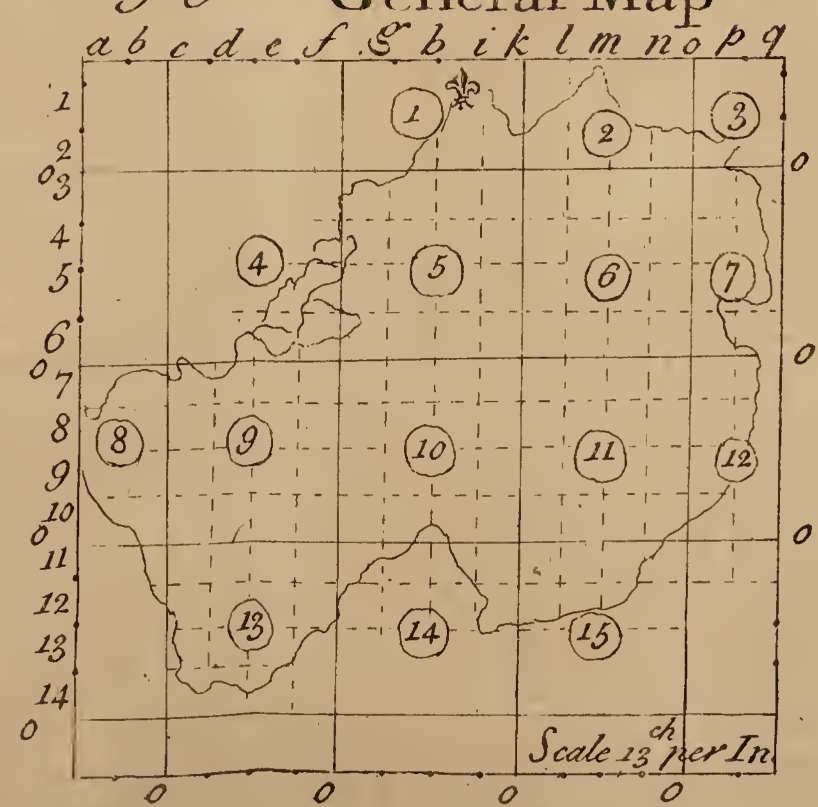


Fig. 91. Particular Map No IV

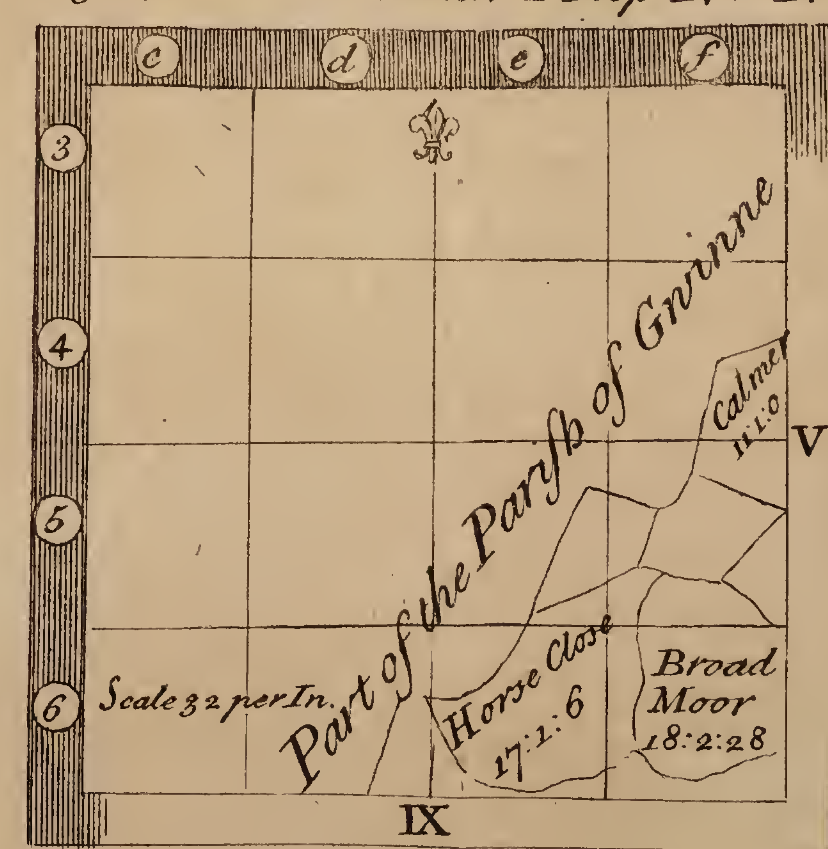
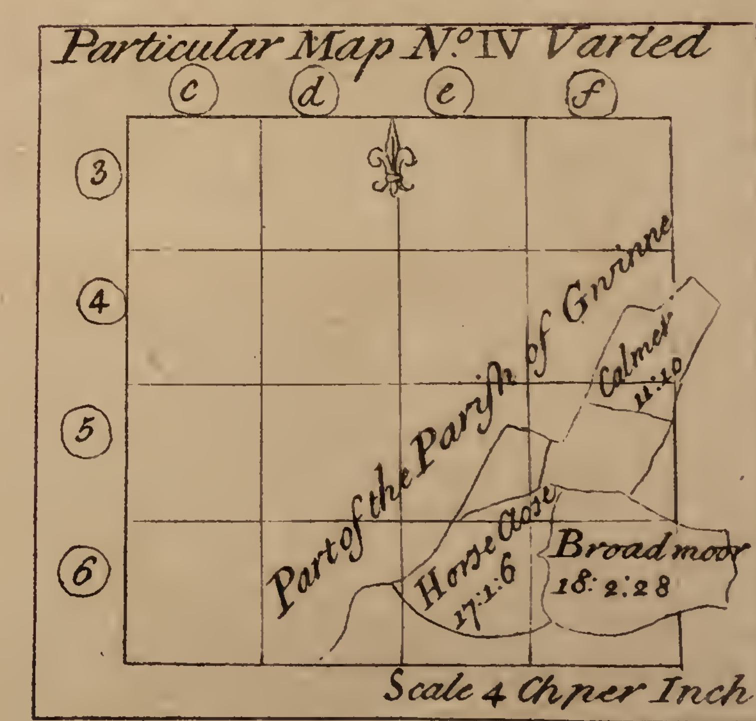


Fig. 92.



The Breadth of the whole Map, by a Scale of 32, is 60,62 Inches, which I would reduce into the Compafs of 16 ¼ and ⅛ = 16,37 Inches.

Divide 60,62 by 16,37, gives 3.7, which multiplied by 3,20, makes the Product 11,84, that is 11 Chains 84 Links in an Inch, the Scale for the general Map.

Thus have I done all I intended; but fhall obferve, that feveral of thefe Tables have been made, and, as People have fanfied, with Alterations and Additions; but all Variations are not really Improvements. The fetting it horizontally by Spirit-Tubes, may be curious enough: But as the Difference is very inconfiderable and indifcernable, when it ftands 2 or 3 Degrees out of the Level, I fhall not trouble myfelf or others about it; only further obferve, that when Grounds are declining much, and very uneven, if the Table ftands horizontal, unlefs the Sight or Mark on the lower Part is fo high as it's Top makes a Level with the upper Part of the Table, which is feldom done, or practicable, I do not fee why fuch a Strefs fhould be laid on the Inftrument's being level, when neither the View by the Index, nor the Meafure of the Line, either can be, or is taken horizontally: If the Sight of the Index ftand nearly perpendicular at every Obfervation, it is more than fufficient for any Exactnefs requifite in a Survey.

CHAP. V.

MECHANICKS.

I. HAVING laft Year fhewn feveral Perfons in *Holland* the Experiment contrived by Mr *Geo. Graham*, to explain the Doctrine relating to the *Momentum* of Bodies (*viz.* That the *Momentum*, or Quantity of Motion in Bodies, is always as the Mafs multiplied into the Velocity) which Experiment is made with a flat, pendulous Body, that receives the Addition of a Weight equal to itfelf at the lower Part of it's Vibration, and by the Reception of that equal Quantity of Matter always lofes half it's Velocity. Dr *Mufchenbroek*, Profeffor of Mathematicks and Aftronomy at *Utrecht*, communicated to me the following Experiment, made in Oppofition to that which I was fhewed by Mr Profeffor *s'Gravefande*. In this laft a Spring equally bent every time, pufhes forward, unequal Quantities of Matter fucceffively, and in every Experiment the Product of the Mafs of the Body by the Square of the Velocity is the fame; and therefore, as the Quantity of Motion muft always be the fame from the fame Caufe (*viz.* the fame Tenfion of the Spring) it follows, by every Experiment, that it is as the Mafs multiplied into the Square of the Velocity.

An Experiment by G. J. s'Gravefande, *Prof. Math.* Leyd. *F. R. S. relating to the Force of moving Bodies, fhewn to the* Royal Society, *by* J. T. Defaguliers, *LL.D. F.R.S.* No. 429. p. 143. July. *&c.* 1733.

Exp. 1.] The pendulous Cylinder is ſhot by the Spring from 0 Deg. to 7 Deg. meaſured upon a Tangent Line.

Exp. 2.] The Cylinder with a leaden Weight in it that makes it's Weight double, is ſhot forward to 4 Deg. $\frac{9}{10}$.

Exp. 3.] The Cylinder with a Weight in it that made it's Weight triple, was ſhot forward to 4 Degrees and a little farther.

Exp. 4.] The Cylinder with a triple Weight of Lead ſo as to quadruple the whole Weight, was ſhot forward to 3 $\frac{1}{2}$ Deg.

Theſe 4 Experiments at firſt ſeem agreeable to the new Hypotheſis; for according to the old, the Cylinder in the 2d Experiment ought to have gone but to 3 $\frac{1}{2}$ Deg. in the 3d Experiment but to 3 $\frac{1}{7}$ Deg. and in the laſt but to 2 Deg.

But if we take in the Conſideration of Time, all will be reduced to the old Principle. As for Example, let us compare the firſt and laſt Experiments.

In the firſt, the Spring during a certain time acts upon the Cylinder which is driven forward with the Velocity 8. When the quadrupled Weight is driven forward with the Velocity 4 inſtead of 2, it is becauſe the ſame Spring acts twice as long upon the Cylinder before it ceaſes to impel it; and certainly the ſame Cauſe acting twice as long muſt produce a double Effect.

A ſhort Account of Dr Jurin's *Ninth and laſt Diſſertation* De Vi Motrice, *by Mr* John Eames, *F. R. S.* No. 459. p. 607. Jan. *&c.* 1741.

II. The laſt Diſſertation is* new, and treats of the Motive Forces of Bodies, whether they are to be eſtimated by the Velocities, or the Squares of the Velocities, when the Maſſes are equal. The Original of this diſpute among the Mathematicians, the Author aſcribes to a Slip committed by the celebrated Mr *Leibniz*, in the Year 1686, and the Continuance, to the Neglect of the Times, wherein equal Effects are produced. The one Side aſſerts all Cauſes to be equal, whoſe Effects are ſo, whether the Times, during which the Cauſes act, are ſhorter or longer. The other, on the contrary, maintains, that equal Effects may ariſe from unequal Cauſes, if the Times of Action are unequal; that conſequently the Times, as well as the Effects, ought to be taken into the Account.

He wiſhes the Gentlemen on the other Side of the Queſtion would produce ſome Experiment in their Favour, where the Equality of the Times is preſerved; ſince all the Experiments they have hitherto made, and argued from, may juſtly be ſet aſide, as incompetent, on the Account of the Inequality of the Times of Action.

The Author then proceeds to prove the Truth of the common Opinion of the Forces in equal Bodies being proportional to their Velocities. This he does by Three Mediums, the Firſt taken from

* The Eight preceding Diſſertations had been before printed ſeparately; but were now all collected together, with the Addition of this Ninth, and publiſhed in one Volume in *Octavo*, *London*, 1732.

the Action of a single Spring upon the same Body: The Second from some Experiments of Mr *Mariotte*; the Third from the joint Action of several Springs upon two unequal Bodies.

I. A single Spring, fixed to a moveable horizontal Table, is made to communicate to the same Body, Degrees of Force unquestionably equal, while the Degrees of Velocity communicated at the same time are also undoubtedly equal; therefore the Forces are proportional to the Velocities.

II. In Mr *Mariotte*'s Experiments, the Impressions made upon equal Surfaces in the same Point of Time, are found to be in the Duplicate *Ratio* of the Velocities; but the Masses or Numbers of impinging Particles are in the simple *Ratio* of the Velocities; consequently, the Masses and Velocities conjunctly being in the Duplicate *Ratio*, *i. e.* as the Impressions, must also be as the Forces which made them: Which is the old Opinion.

III. A complicated or bent Spring interposed between two unequal Bodies, acting upon each with an equal Pressure, and during an equal Time, must communicate equal moving Forces to each; but their Velocities are by Experiment reciprocally proportional to their Masses; therefore their Masses, drawn into their respective Velocities, are also equal, as were their moving Forces; and by consequence their moving Forces are as the Masses and Velocities conjunctly: Which is the generally received Opinion.

In the Appendix, the Author answers some of the principal Arguments brought in favour of the contrary Side.

I. The first is drawn from the compound Motion of a Body along the Diagonal of a Rectangle, whose Sides represent the simple Motions. Here it is said, that the simple Forces are no-ways contrary to each other; that being united or added together in the compound Force, that compound Force will not be to both or either of the simple Forces, as the Diagonal is to both or either of the Sides; but as the Square of the Diagonal to the Sum of the Squares of the Sides, or to the Square of either Side respectively. He answers, The simple Forces, while they act in their proper Directions, are not contrary to each other, either Wholly or in Part; but when considered as contributing to the Motion of the Body in the Direction of the Diagonal, Part of the one acts contrary to Part of the other, and destroys it; as is evident, if you resolve each simple Force into two others, one acting along the Diagonal, the other in a Direction perpendicular to it. And then it is to be observed, that the Sum of the two former is equal to the Diagonal (while the two latter destroy each other): Which is perfectly agreeable to the old Opinion, but not at all to the new; for the demonstrating of which this Argument is brought.

II. The second Proof is taken from the equal Compression of 4 equal Springs, before the Force was consumed, by the same Body moving with

with double the Velocity; and labours at the Bottom under the fame Parallogifm.

III. The laft Argument is founded upon the learned and ingenious Mr *Poleni*'s Experiment, wherein equal Cavities are formed in foft Subftances, by equal Bodies falling from Heights reciprocally proportional to their Maffes. This the Author fets afide, as infufficient; fince the Times of forming thefe equal Cavities are unequal, and unequal Caufes may produce equal Effects in unequal Times. The learned Mr *Poleni* does, indeed, reply, and fay, that the Formation of thefe Cavities feems to be inftantaneous: But the ingenious Author fhews the contrary, and that from a Pofition allowed of by *Poleni* himfelf, in his Reply.

Obfervations made in London, *by Mr* George Graham, *F.R.S. and at* Black-River *in* Jamaica, *by* Colin Campbell, *Efq; F. R. S. concerning the Going of a Clock; in order to determine the Difference between the Lengths of* Ifochronal Pendulums *in thofe Places. Communicated by* J. Bradley, *M. A. Aftr. Prof. Savill.* Oxon. *F. R. S.* No. 432. p. 302. Apr. &c 1734.

III. 1. Altho' it is now above 60 Years fince Mr *Richer* firft difcovered, that Pendulums of the fame Length, do not perform their Vibrations in equal Times in different Latitudes; and tho' feveral Experiments made fince in different parts of the Earth concur to prove, that Pendulums fwinging Seconds are in general fhorter as we approach the Equator; yet what the real Difference is between their Lengths in different Latitudes, does not feem to have been determined with fufficient Exactnefs, by the Obfervations that have hitherto been communicated to the Publick; as may be gathered from Sir *I. Newton*'s *Principia* *, where they are compared as well with each other, as with the Theory of that illuftrious Author. It were therefore to be wifhed, that more of this kind of Experiments could be made with greater Accuracy in proper Places, by fuch Perfons as have fufficient Skill and Opportunities to do it; that we might thereby be enabled to judge with more Certainty, concerning the true Figure of the Earth, and the Nature of it's conftituent Parts.

As an Inducement to fuch as may have it in their Power to put the like again into Practice, I fhall lay before the Society, an Account of a very curious Experiment of this Sort lately made in *Jamaica*, by *Colin Campbell*, Efq; He has furnifhed himfelf with an Apparatus of Inftruments not unworthy the Obfervatory of a Prince; among which is a Clock whofe Pendulum vibrates Seconds, made by Mr *George Graham*, who judging that an Opportunity was now offered of trying with the utmoft Exactnefs, what is the true Difference between the Lengths of Ifochronal Pendulums at *London* and *Jamaica*, readily embraced it; and in framing the Parts of the Clock, carefully contrived, that it's Pendulum might at pleafure be reduced to the fame Length, whenever there fhould be occafion to remove the Clock from one Place, and fet it up in another.

This Clock being chiefly defigned for Aftronomical Obfervations, had no ftriking Part, and it's Pendulum was adjufted to fuch a Length, that in *London* is vibrated Seconds, of Siderial, and not of Solar Time.

* Lib. III. Prop. 20.

When

When it was finiſhed, Mr *Graham* fixed it up in a Room ſituated backward from the Street, and on the North-ſide of his Houſe, to prevent it's being diſturbed by Coaches, or other Carriages that paſſed through the Street, and that it might be as little affected by the Sun as poſſible. Having ſet it going, he compared it with the Tranſits of the Star *Lucida Aquilæ* over the Meridian, which paſſed

	th		h.	′	″	
Auguſt, 1731.	20	at	8	59	15	by the Clock.
	22	at	8	59	18	
	23	at	8	59	20½	
	25	at	8	59	22	
	28	at	8	59	25½	
	29	at	8	59	26	
	30	at	8	59	27	

Hence it appears, that the Clock gained 12″ in 10 Apparent Revolutions of the Star.

In order to eſtimate how much the Pendulum may be lengthened by greater Degrees of Heat, or how much ſlower the Clock would go on that Account when removed into a warmer Climate, a Thermometer was fixed by the Side of it; and between the Hours of 10 and 11 in the Morning, and at Night, notice was taken at what Height the Spirits ſtood, and the mean Height for each Day was as follows:

	th	*Therm.*
Auguſt, 1731.	21	32 ½ Diviſions.
	22	30 ¼
	23	28 ¾
	24	27 ¾
	25	28 ¼ Hence the mean Height for all theſe Days was about 28 ⅓
	26	27 ¼ Diviſions.
	27	27 ½
	28	27 ½
	29	27 ½
	30	27 ¾

The Clock-Weight that keeps the Pendulum in Motion is 12 ℔. 10½ *oz.* and is to be wound up once in a Month. The Weight of the Pendulum itſelf is 17 ℔. and (during the Time that the Clock was compared with the Tranſits of the Star) it vibrated each way from the Perpendicular 1° 45′. The Magnitude of the Vibrations was eſtimated by means of a Braſs Arc, which was fixed juſt under the lower end of the Rod of the Pendulum, and divided into Degrees, *&c.*

August 31, Mr. *Graham* took off the Weight belonging to the Clock, and hung on another of 6 ℔. 3 *oz.* and with this Weight the Pendulum vibrated only 1°. 15′ on each Side; and the Clock went 1″ ½ ſlower in 24 Hours, than when it's own Weight of 12 ℔. 10 ½ *oz.* was hung on.

This Experiment ſhews, that a ſmall Difference in the Arcs deſcribed by the Pendulum, or a ſmall Alteration in the Weight that keeps it in Motion, will cauſe no great Difference in the Duration of the Vibrations; and therefore a little Alteration in the Tenacity of the Oil upon the Pivots, or in the Foulneſs of the Clock, will not cauſe it to accelerate or retard it's Motion ſenſibly; from whence we may conclude, that whatever Difference there ſhall appear to be, between the going of the Clock at *London* and in *Jamaica*, it muſt wholly proceed from the lengthening of the Pendulum by Heat, and the Diminution of the Force of Gravity upon it.

A particular written Account of the Obſervations and Experiments hitherto taken Notice of, was delivered to me by Mr *Graham* in *Sept.* 1731, about the ſame Time the Clock was put on Ship-board to be carried to *Jamaica*. He likewiſe ſent very full Directions to Mr *Campbell*, deſcribing in what manner the Clock was to be fixed up, and how the Pendulum might be reduced exactly to the ſame State as it was when in *England*; but no Intimation was given concerning the going of the Clock, that the Experiment might be made with all poſſible Care, and Caution, and without any Byaſs, or Prejudice, in Favour of any Hypotheſis, or former Obſervations.

In *July* 1732, we received an Account of the Succeſs of the Experiment, by the Hands of Mr *Joſeph Harris*, who was preſent at the making of it in *Jamaica*, whither he went the Year before with Mr *Campbell*, in order to aſſiſt him in his Deſign of erecting an Obſervatory for the Improvement of Aſtronomy, and the promoting other Parts of Natural Knowledge in that Iſland: But his ill State of Health obliging him to return into *England*, he brought with him the Original Journal of the Obſervations of the Tranſits of two Stars (*viz. Syrius* & β *Canis Majoris*) over the Meridian, compared with the Clock, after it was fixed up in *Jamaica*, as Mr *Graham* had directed; together with the Height of the Spirits of the forementioned Thermometer, upon the ſeveral Days of Obſervation.

The chief of thoſe Obſervations are contained in the following Table, the 1ſt Column whereof ſhews the Day of the Month; the 2d, the Name of the Star, and the Time by the Clock of it's obſerved Tranſit over the Meridian; the 3d contains the Hour of the Day, when the Thermometer was obſerved, together with the Height of the Spirit at thoſe Hours; the Morning Hours being denoted by the Letter A, and thoſe of the Afternoon, by the Letter P.

1732

1732	Canis Majoris.	Time of Transit. h	′	″	Hour of Day. h		Thermometer.
Jan. 23	β	11	59	50	10½	A	14¾
	α	12	22	14	9½	P	11
24	Cloudy.				11½	A	15¼
25	β	11	55	40	8½	A	17½
	α	12	18	4	9¼	P	11¼
26	β	11	53	35	8	A	20
	α	12	16	00	2	P	8½
					9	P	10
27	β	11	51	31	7	A	17½
	α	12	13	55	2	P	8½
					9¼	P	12½
28	β	11	49	26	7	A	20½
	α	12	11	51	2	P	11
					10	P	12
29	β	11	47	22	6¾	A	19
	α	12	9	46	3	P	9
					9	P	11¼
30	Cloudy.				7	A	20½
					4	P	7
					11	P	13
31	β	11	43	12	7	A	20
	α	12	5	37	9	P	8½
Feb. 1	β	11	41	8½	10	A	18¾
	α	12	3	33	11	P	16
2	β	11	39	0	9½	A	17½
	α	12	1	23½	2	P	9
					5	P	6
					9	P	8½
3	β	11	36	53	8½	A	19
					1	P	9½
					9	P	9
4	β	11	34	46	6¼	A	18
	α	11	57	11	12		9½
					9	P	8
Feb. 5	β	11	32	40	7½	A	19¼
	α	11	55	5	3½	P	6
					8½	P	87
6	β	11	30	35	7	A	18½
	α Cloudy.				4	P	7½
					8½	P	8
7	β	11	28	31	7	A	20½
	α	11	50	55	12		12
					8½	P	8½
8	β Cloudy.				6½	A	21½
	α	11	48	50	8½	P	8¼
9	β	11	24	20	9½	A	14
	α	11	46	44	8½	P	8
10	β	11	22	12½	7¼	A	16
	α	11	44	37	11½	A	10
					3¼	P	3¼
					8½	P	6
11	β	11	20	6	7½	A	16
	α	11	42	30	12		9½
					8¼	P	5¾
12	β	11	18	0	10	A	17½
	α	11	40	24	12		13
					8	P	5¾
13	Cloud.				9	A	17
					8	P	6
14	β Cloudy.				7½	A	16
	α	11	36	15	12		11
					8	P	10
15	Clouds.				9	A	18
					12		13¼
					8½	P	7½
16	β Cloudy.				8	A	14
	α	11	32	4	8	P	7
17	β	11	7	34	12		12
	α	11	29	59	8	P	6¼
18	β	11	5	29	12		12¼
	α	11	27	53			

The Pendulum, during this Interval, vibrated about 1° 52′ each way from the Perpendicular.

The Tranſits of the Stars over the Meridian, were obſerved with a Teleſcope, fixed at Right Angles to an Horizontal Axis, whoſe Ends lay exactly Eaſt and Weſt; by the turning of which Axis, the Line of Collimation of the Teleſcope, was conſtantly directed in the Plain of the Meridian. This Inſtrument was daily adjuſted to a Mark, fixed in the Meridian: and in the Journal, between the 2d and 3d of *February*, the following Remark was made.

N. B. *This Day was hotter than uſual, as appears by the Thermometer; and the Tranſit Inſtrument had loſt the Level a little, but after we had adjuſted it, it pointed exactly to our Meridian Mark, and therefore we are at a loſs for the Cauſe of this Difference in the Clock.*

From the foregoing Table it appears, that the Clock loſt 54′ 21″ in 26 Revolutions of the Stars; that is, about 2′ 5″½ in one Revolution, the Difference from this Medium ſomewhat varying, upon account of a greater, or leſs Degree of Heat on different Days.

The Mean of all the obſerved Heights of the Thermometer from *January* 26th, to *February* 18th, was about 12½ Diviſions. Therefore, the Difference between the mean Heights of the Thermometer, at *Jamaica* and *London*, during the Intervals of the reſpective Obſervations, was 15½ Diviſions; the Spirits ſtanding ſo much higher in *Jamaica*, becauſe of the greater Heat in that Iſland.

That we might be able to judge, how much the different Degrees of Heat, correſponding to any Number of Diviſions upon this Thermometer, would cauſe the Clock to go ſlower, by lengthening it's Pendulum, Mr *Graham* took Notice of the loweſt Point, to which the Spirits ſunk at *London* in the Winter, 1731; and the greateſt Height to which they roſe in the following Summer; and comparing the Motion of the Spirits in this Thermometer, with the Alterations in another made with Quickſilver, which he has for ſome Years made uſe of; he concluded, that at *London* the Spirits in this Thermometer would ſtand (*communibus Annis*) about 60 Diviſions higher in Summer than in Winter.

By ſeveral Years Experience, he has likewiſe found, that his Clocks (of the ſame ſort with Mr *Campbell*'s) when expoſed, as uſual, to the different Degrees of Heat and Cold of our Climate, do not vary in their Motion above 25 or 30 Seconds in a Day.

From theſe Obſervations and Experiments therefore we may reaſonably conclude, that ſufficient Allowance will be made for the Lengthening of the Pendulum by Heat, if we ſuppoſe the Clock, upon that Account, to go one Second in a Day ſlower, when the Spirits of this Thermometer ſtand two Diviſions higher, and in the ſame Proportion for other Heights.

Admitting then, that the mean Height of the Thermometer, while the Clock was compared with the Stars at *Jamaica*, exceeded that at *London*

London between 15 and 20 Divifions; if we allow 8, or 9 Seconds, upon that Account, the remaining Difference muft be wholly owing to the Difference of the Force of Gravity in the two Places.

Upon comparing the Obfervations, it appears, that in one apparent Revolution of the Stars, the Clock went $2' \, 6'' \frac{1}{2}$ flower in *Jamaica*, than at *London*; deducting therefore $8'' \frac{1}{2}$, on account of the greater Heat in *Jamaica*, there remains a Difference of $1' \, 58''$, which muft neceffarily arife from the Diminution of Gravity, in the Place neareft the Equator.

I have allowed the Clock to have loft fomewhat more, on account of the Difference of Heat, than the mean Heights of the Thermometer may feem to require, upon a Suppofition, that the total Heat of the Days, compared with the Cold of the Nights, bears a greater Proportion in *Jamaica*, than *London*; but if that Suppofition be not admitted, then the Clock in *Jamaica*, muft have gone rather more than $1' \, 58''$ in a Day flower than in *England*.

Mr *Campbell*'s Obfervations were made at *Black-River*, in 18° North Latitude. Now if we fuppofe, with Sir *I. Newton*, that the Difference in the going of the Clock, is owing to the greater Elevation of the Parts of the Earth towards the Equator, it will follow from thefe Obfervations, and what is delivered by him in *Lib.* III. *Prop.* 20. of his *Principia*, that the Æquatorial Diameter is to the Polar, as 190 to 189; the Difference between them being $41\frac{1}{2}$ Miles; which is fomewhat greater than what Sir *I. Newton* had computed from his Theory, upon the Suppofition of an uniform Denfity in all the Parts of the Earth.

I fhall not enter into the Difpute about the Figure of the Earth, but at prefent fuppofe, with Sir *I. Newton*, that the Increafe of Gravity, as we recede from the Æquator, is nearly as the Square of the Sine of the Latitude; and that the Difference in the Length of Pendulums, is proportional to the Augmentation, or Diminution of Gravity. Upon thefe Suppofitions, I collect from the forementioned Obfervations, that, if the Length of a fimple Pendulum (that fwings Seconds at *London*) be 39.126 *Englifh* Inches, the Length of one at the Æquator, would be 39.00, and at the Poles 39.206. And (abftracting from the Alteration on account of different Degrees of Heat) a Pendulum-Clock that would go true Time under the Æquator, will gain $3' \, 48'' \frac{1}{4}$ in a Day at the Poles; but the number of Seconds which it would gain in any other Latitude, would be to $3' \, 48'' \frac{1}{4}$ nearly, as the Square of the Sine of that Latitude is to the Square of the Radius: From whence it follows; that the Number of Seconds which a Clock will lofe in a Day, upon it's Removal to a Place nearer to the Æquator, will be to $3' \, 48'' \frac{1}{4}$ nearly, as the Difference between the Squares of the Sines of the Latitudes of the two Places to the Square of the Radius. Thus the Difference of the Squares of the Sines of $51° \frac{1}{2}$, and 18°, the Latitudes of *London* and *Black-River* being to

the Square of the Radius, as 118 to 228¼, the Clock will go 1′ 58″ in a Day slower at *Black-River* than at *London*, as was found by Observation.

It may be hoped, that Mr *Campbell*'s Success in this Experiment, and the little Trouble there is in making it, will induce those Gentlemen who may hereafter carry Pendulum-Clocks into distant Countries, to attempt a Repetition of it after his manner; that is, by keeping or restoring the Pendulums of their Clocks to the same Length in the different Places, and carefully comparing them with the Heavens; and at the same Time taking notice of the different Degrees of Heat, by means of a Thermometer. From a Variety of such Experiments, we should be enabled to determine how far Sir *I. Newton*'s Theory is conformable to Truth, with much greater Certainty than from those Trials which are made by actually measuring the Lengths of simple Pendulums; because a Difference of $\frac{1}{100}$ Part of an Inch, in the Length of a Pendulum, corresponds to 11″ in a Day; and it being easy to observe how much a Clock gains, or loses in a Day, even to a single Second; it is certain, that by means of a Clock, compared in the manner above-mentioned, we may distinguish a Difference (in the Lengths of Isochronal Pendulums) of $\frac{1}{1000}$ Part of an Inch, or less; whereas it will be scarce possible to measure their true Lengths, without being liable to a greater Error than that. Besides, by taking Notice how much a Clock gains, or loses, upon the falling or rising of a Thermometer, we can better allow for the different Degrees of Heat in this, than in the other Method of making the Experiment, by actual Measurement; since it may not be easy to determine how much the Measure itself, which we make use of, will be lengthened by different Degrees of Heat.

For these Reasons, I esteem Mr *Campbell*'s Experiment to be the most accurate of all that have hitherto been made, and properest to determine the Difference of the Gravity of Bodies in different Latitudes; and therefore I shall subjoin a Table, which I computed from it, containing the Difference of the Length of a simple Pendulum, swinging Seconds at the Æquator, and at every 5th Degree of Latitude, together with the Number of Seconds that a Clock would gain in a Day, in those several Latitudes, supposing it went true, when under the Æquator; by means of which any one may readily compare other the like Observations with his; and thereby discover whether the Alteration of Gravity in all Places be uniform, and agreeable to the Rule laid down by Sir *I. Newton* or not.

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The Latitude of the Place.	The Difference of the Length of the Pendulum in Parts of an *English* Inch.	Seconds gained by a Clock in one Day.	The Latitude of the Place.	The Difference of the Length of the Pendulum in Parts of an *English* Inch.	Seconds gained by a Clock in one Day.
Deg.	Inch.	Seconds.	Deg.	Inch.	Seconds.
5	0. 0016	1. 7	50	0. 1212	134. 0
10	0. 0062	6. 9	55	0. 1386	153. 2
15	0. 0138	15. 3	60	0. 1549	171. 2
20	0. 0246	26. 7	65	0. 1696	187. 5
25	0. 0369	40. 8	70	0. 1824	201. 6
30	0. 0516	57. 1	75	0. 1927	213. 0
35	0. 0679	75. 1	80	0. 2003	221. 4
40	0. 0853	94. 3	85	0. 2050	226. 5
45	0. 1033	114. 1	90	0. 2065	228. 3

2. The preceding Article brought to my Mind ſome Experiments I made ſome Years ago, that may be of Uſe in Obſervations of this Nature.

Experiments concerning the Vibrations *of* Pendulums. *By the late* W. Derham, *D. D. F. R. S. and Canon of* Windſor. No. 440. p. 201. Jan. *&c.* 1736.

The firſt that I ſhall take notice of, ſhall be ſome Experiments I made in the Year 1704, with excellent Inſtruments, concerning the *Vibrations of Pendulums in Vacuo* *. The Sum of which is, That the Vibrations in *Vacuo* were larger than in the *open Air*, or Receiver unexhauſted: Alſo that the Enlargement or Diminution of the Vibrations, was conſtantly in Proportion to the Quantity of Air, or Rarity, or Denſity thereof, which was left in the Receiver of the Air-Pump. And as the Vibrations were larger or ſhorter, ſo the Times were augmented, or diminiſhed accordingly; *viz.* 2'' in an Hour ſlower, when the Vibrations were largeſt, and leſs and leſs, as the Air was re-admitted, and the Vibrations ſhortened.

But notwithſtanding the Times were ſlower, as the Vibrations were larger, yet I had Reaſon to conclude, that the Pendulum really moved quicker in *Vacuo*, than in the *Air*, becauſe the ſame Difference, or Enlargement of the Vibrations (as two Tenths of an Inch on a Side) would cauſe the Movement, inſtead of 2'' in an Hour to go 6 or 7'' ſlower in the ſame Time; as I found by nice Experiments.

The next Experiments I ſhall mention, I made at ſeveral Times, in 1705, 1706, and 1712, by the Help of a good Month Piece that ſwings Seconds. The Weight that then drove it, was about 12 or 13 ℔, and it kept Time exactly by the Sun's mean Motion: But by hanging on 6 ℔ more, the Vibrations were enlarged; yet the Clock gained but 13 or 14'' in a Day.

And as the Increaſe or Diminution of the Power that drives the Clock, doth accelerate or retard it's Motion; ſo, no doubt, doth

* See Vol. IV. Part II. Chap. I. §. 32.

Cleanneſs

Cleanneſs or *Foulneſs* affect it, and ſo doth *Heat* and *Cold*; for all have the ſame Effect upon the Pallets and Pendulum.

The laſt Experiments I ſhall mention, I made in 1716 and 1718, to try what Effects *Heat* and *Cold* had upon *Iron Rods* of the ſame Length, or as near as I could to thoſe that ſwing Seconds. I made my Experiments with round Rods of about $\frac{1}{4}$ of an Inch Diameter, and with ſquare Rods, of about $\frac{3}{4}$ of an Inch Square. The Effects on both which were the ſame.

At firſt I took the exact Length of the Rods, in their natural Temper. Then I heated them as well as I could in a Smith's Fire, from End to End nearly to a *Flaming Heat*; by which means, they were lengthened $\frac{2}{10}$ of an Inch. Then I *quenched* them in cold Water; which made them $\frac{[illegible]}{100}$ of an Inch ſhorter than in their natural State.

Then I warmed them to (as near as I could gueſs) the *Temper* of my Body; by which means they were about $\frac{1}{100}$ of an Inch longer than in their natural Temper.

Afterwards I cooled them in a *ſtrong frigorifick Mixture* of common Salt and Snow, which ſhortened them $\frac{3}{100}$ Parts of an Inch.

Afterwards I meaſured theſe Rods, when heated in an *hot Sun*, which lengthened them $\frac{2}{100}$ Parts of an Inch more than their natural Temper.

All theſe Experiments ſeem to concur in reſolving the Phænomenon of *Pendulum-Clocks going ſlower under the Æquator* than in the Latitudes from it: But yet I confeſs, that I have too good an Opinion of Sir *I. Newton*'s Notion of the Sphæroidal *Figure* of the *Earth*, to part eaſily with it; and therefore I leave it to the Conſideration of others, how far the Figure of the Earth, and how far Heat and Cold, and the Rarity and Denſity of the Air, are concerned in that Phænomenon.

An Account of the Influence which two Pendulum-Clocks were obſerved to have upon each other, by Mr John Ellicott, *F. R. S.* No. 453. p. 126. April *&c.* 1739.

IV. 1. The two Clocks upon which the following Obſervations were made, being deſigned for Regulators, particular Care was taken to have every Part made with all poſſible Exactneſs: The two Pendulums were hung in a manner different from what is uſual; and ſo diſpoſed, that the Wheels might act upon them with more Advantage. Upon Trial they were found not only to move with greater Freedom than common, but an heavier Pendulum was kept in Motion by a ſmaller Weight. They were in every reſpect made as near alike as poſſible. The Ball of each of the Pendulums weighed above 23 ℔; and required to be moved about 1° 5′ from the Perpendicular, before the Teeth of the ſwing Wheel would ſcape free of the Pallets; that is, before the Clocks would be ſet a-going. The Weight to each was 3 ℔, which would cauſe either of the Pendulums, in their Vibrations to deſcribe an Arch of 3°. The two Clocks were each in Caſes, which ſhut very cloſe, and placed Sideways to one another, ſo near that when the Pendulums were at Reſt, they were little more than about 2 Feet aſunder. The odd *Phænomena* obſerved in them were theſe: In leſs than 2 Hours after they were ſet a-going, one of them (which I call N° 1.) was found to ſtop; and

and when ſet a-going again, (as it was ſeveral times) would never continue going two Hours together. As it had always kept going with great Freedom, before the other Clock (which I call N° 2.) was placed near it, this led me to conceive it's ſtopping muſt be owing to ſome Influence the Motion of one of the Pendulums had upon the other; and upon watching them more narrowly, I found the Motion of N° 2. to increaſe as N° 1. diminiſhed; and at the time N° 1. ſtopped, N° 2. deſcribed an Arch of 5°, that is nearly 2° more than it would have done, if the other had not been near it, and more than it did move in a ſhort time after the other Pendulum came to be at Reſt: This made me imagine that they had a mutual Influence upon each other. Upon this I ſtopped the Pendulum of N° 2. leaving it quite at Reſt, and ſet N° 1. a-going, the Pendulum deſcribing as large an Arch as the Caſe would permit, *viz.* about 5°. In about 20 Minutes after, I went to obſerve whether there was any Motion communicated to the Pendulum N° 2. when, to my great Surprize, I found the Clock going, and the Pendulum to deſcribe an Arch of 3°, whereas at the ſame time N° 1. did not move 4°. In about half an Hour after, N° 1. ſtopped, and the Motion of N° 2. was increaſed to very near 5°. I then ſtopped N° 2. a ſecond time, and ſet N° 1. a-going, as before; and ſtanding to obſerve them, I preſently found the Pendulum of N° 2. to begin to move, and the Motion to increaſe gradually, till in 17′ 40″ it deſcribed an Arch of 2° 10′, at which time the Wheel diſcharging itſelf of the Pallets, the Clock went. The Arches of the Vibrations continued to increaſe, till (as in the former Experiment) the Pendulum moved 5°; the Motion of the Pendulum N° 1. gradually decreaſing all the while, as the other increaſed; and in three Quarters of an Hour after, it ſtopped. I then left the Pendulum of N° 1. at Reſt, and ſet N° 2. a-going, making it deſcribe an Arch of 5°; it continued to vibrate leſs and leſs, till it deſcribed but about 3°; in which Arch it continued to move all the time I obſerved it, which was ſeveral Hours. The Pendulum of N° 1. ſeemed but little affected by the Motion of N° 2. I tried theſe Experiments ſeveral times over, without finding any remarkable Difference. The freer the Room was from any Motion (as Peoples walking about in it, &c.) I found the Experiments to ſucceed the better; and once I found N° 2. ſet a-going in 16′ 20″, and N° 1. at that time ſtopped in 36′ 40″.

2. In my former Account I took Notice, that the two Clocks were in ſeparate Caſes, and that the Backs of them reſted againſt the ſame Rail; that the Pendulums, when at Reſt, were about 2 Feet aſunder, and weighed about 23 ℔ each, and were made to move with ſuch Freedom, that a Weight of 3 ℔ would cauſe either of the Pendulums to deſcribe an Arch of three Degrees. The moſt remarkable Particulars then obſerved in them were theſe: If the Pendulum of one of the Clocks, which (for Diſtinction ſake) I called N° 2. was left at Reſt, and that of the other, which I called N° 1. was ſet a-going, this would, in about 16 Minutes,

—Further Obſervations and Experiments; by the ſame. Ibid. p. 128.

16 Minutes, communicate fo great a Quantity of Motion to N° 2. as would make it's Pendulum defcribe an Arch of above two Degrees, and would fet the Work a-going: That the Motion of the Pendulum of N° 1. conftantly decreafed as that of N° 2. increafed, and after about 30 Minutes it did not defcribe an Arch fufficient to free the Teeth of the Wheel from the Pallets, fo that the Clock ftopped. At the fame time the Pendulum of N° 2. defcribed an Arch of five Degrees, which was two Degrees more than it would have done, had it not been affected by the Motion of N° 1. Upon leaving the Pendulum of N° 1. at Reft, and fetting N° 2. a-going, the Pendulum of N° 1. was found to be but little affected, and never moved fufficiently to fet the Work a-going. Thefe feemingly different Effects, which the two Clocks had upon each other, I fhall now endeavour to account for.

The Manner in which the Motion is communicated to the Pendulum at Reft, I conceive to be thus: As the Pendulums are very heavy, when either of them is fet a going, it occafions by it's Vibrations a very fmall Motion, not only in the Cafe the Clock is fixed in, but, in a greater or leffer Degree, in every thing it touches; and this Motion is communicated to the other Clock, by means of the Rail, againft which both the Cafes bear. The Motion thus communicated, which is too fmall to be difcovered but by means of fome fuch-like Experiments as thefe, will, I doubt not, be judged by many, infufficient to make fo heavy a Pendulum defcribe an Arch of 2°, or large enough to fet the Work a-going; and indeed it would be fo, but for the very great Freedom with which the Pendulum is made to move, arifing from the Manner in which it is hung. This appears from the very fmall Weight required to keep it going, which, when the Clock was firft put together, was little more than one ℔. And if the Weight was taken off, and the Pendulum made to fwing two Degrees, it would make 1200 Vibrations before it decreafed half a Degree, fo that it would not lofe the $\frac{1}{3000}$ part of an Inch in each Vibration. Indeed if the Weight was hung on, the Friction would be increafed, and the Pendulum would not move quite fo freely; but even in that Cafe it was found to lofe but little more than the $\frac{1}{2000}$ part of an Inch, or about three Seconds of a Degree, in one Vibration; and therefore if the Motion communicated to it from the other, will make it defcribe an Arch exceeding $3''$, the Vibrations muft continually increafe till the Work is fet a-going. And that the Motion is communicated in the manner above fuppofed, is confirmed by the following Experiments:

A Prop was fet againft the Back of the Cafe of N° 2. to prevent it's bearing againft the Rail; and N° 1. was fet a-going; then obferving them for feveral Hours, I could not perceive the leaft Motion communicated to N° 2. I then fet both the Clocks a-going, and they continued going feveral Days; but I could not find they had any Influence upon each other. Inftead of the Prop againft the Back of the Cafe, I put Wedges under the Bottoms of both the Cafes, to prevent their bearing againft the Rail; and ftuck a Piece

a Piece of Wood between them, juſt tight enough to ſupport it's own Weight. Then ſetting N° 1. a-going, I found the Influence ſo much increaſed, that N° 2. was ſet a-going in leſs than ſix Minutes, and N° 1. ſtopped in about ſix Minutes after. In order to try what Difference would ariſe, if the Clocks were fixed on a more ſolid Floor, I placed them (exactly in the ſame manner as in the laſt Experiment) upon the Stone Pavement under the Piazza's of the *Royal Exchange*, and ſtuck the Piece of Wood between them, as before; and ſetting N° 1. a-going, the only Difference I could perceive, was, that it was 15 Minutes before N° 2. was ſet a-going, and N° 1. continued going near half an Hour before it ſtopped. From theſe Experiments I think it plainly appears, that the Pendulum which is put in Motion, as it moves towards either ſide of the Caſe, makes the Preſſure upon the Feet of the Caſe to be unequal, and, by it's Weight, occaſions a ſmall Bearing or Motion in the Caſe on that Side towards which the Pendulum is moving; and which, by the Interpoſition of any ſolid Body, will be communicated to the other Clock, whoſe Pendulum was left at Reſt. The only Objection to this, I conceive, is the different Effects which the two Pendulums ſeemed to have upon each other. But this I hope to explain to Satisfaction.

For, notwithſtanding theſe different Effects, I ſoon found, by ſeveral Experiments, that the two Clocks mutually affected each other, and in the ſame Manner, though not with equal Force; and that the Varieties obſerved in their Actions upon each other, aroſe from the unequal Lengths of their Pendulums only.

For, upon moving one of the Clocks to another Part of the Room, and ſetting them both a-going, I found that N° 2. gained of N° 1. about one Minute 36 Seconds in 24 Hours. Then fixing both againſt the Rail, as at firſt, I ſet them a-going, and made the Pendulums to vibrate about four Degrees; but I ſoon obſerved that of N° 1. to increaſe and that of N° 2. to decreaſe; and in a ſhort time it did not deſcribe an Arch large enough to keep the Wheels in Motion. In a little time after it began to increaſe again, and in a few Minutes it deſcribed an Arch of two Degrees, and the Clock went. It's Vibrations continued to increaſe for a conſiderable Time, but it never vibrated four Degrees, as when firſt ſet a-going. Whilſt the Vibrations of N° 2. increaſed, thoſe of N° 1. decreaſed, till the Clock ſtopped, and the Pendulum did not deſcribe an Arch of more than one Degree 30 Minutes. It then began to increaſe again, and N° 2. decreaſed, and ſtopped a ſecond time, but was ſet a-going again, as before. After this N° 1. ſtopped a ſecond time, and the Vibrations continued to decreaſe till the Pendulum was almoſt at Reſt. It afterwards increaſed a ſmall matter, but not ſufficiently to ſet the Work a-going. But N° 2. continued going, it's Pendulum deſcribing an Arch of about three Degrees.

Finding them to act thus *mutually* and *alternately* upon each other, I ſet them both a-going a ſecond time, and made the Pendulums

describe as large Arches as the Cases would permit. During this Experiment, as in the former, I sometimes found the one, and at other times the contrary Pendulum to make the largest Vibrations. But as they had so large a Quantity of Motion given them at first, neither of them lost so much during the Period it was acted upon by the other, as to have it's Work stopped, but both continued going for several Days without varying one Second from each other; though when at a Distance, as was before observed, they varied one Minute 36 Seconds in 24 Hours. Whilst they continued thus going together, I compared them with a third Clock, and found that N° 1. went 1′ 17″ faster, and N° 2. 19″ slower, than they did when placed at a Distance, so as to have no Influence upon each other.

Upon altering the Lengths of the Pendulums, I found the Period in which their Motions increased and decreased, by their mutual Action upon each other, was changed; and would be prolonged as the Pendulums came nearer to an Equality, which from the Nature of the Action it was reasonable to expect it would. This discovers the Reason why the Pendulum of N° 2. when left at Rest, would be set a-going by the Motion of N° 1. whereas if N° 1. was left at Rest, it would not be set a-going again by the Motion of N° 2.

For I found by several Experiments, that the same Pendulum, when kept in Motion by a Weight, would go faster, than when it only moved by it's own Gravity. On this Principle, which may easily be accounted for, it follows, that during the Time in which the shortest Pendulum, N° 2. was only acted upon by N° 1. it would move slower, and the Times of it's Vibrations approach nearer to an Equality with those of N° 1. than after it came to be kept in Motion by the Weight; and by this means the Time which N° 1. would continue to act upon it, would be prolonged, and be more than was required to make the Pendulum describe an Arch sufficient to set the Work a-going. But on the contrary, while the Pendulum of N° 1. which was the longest, was only acted upon by N° 2. as it would move slower, the Difference of the Times of the Vibrations would be increased; and consequently the Time which N° 2. would continue to act upon it, would for this Cause be shortened, so that before the Pendulum of N° 1. would describe an Arch sufficient to set the Work a-going, the Period of it's being acted upon would be ended, and it would begin to act upon N° 2. at which time it's Vibrations would immediately decrease, and continue to do so till it came to be almost at Rest. And thus it would continue sometimes to move more, and at other times less, but never sufficiently to set the Clock a-going.

Some Considerations, whether Pendulums are disturbed by any centrifugal Force; by Jo.

V. The Method used in discovering the centrifugal Force has always been, to compare Observations made in Countries lying at a vast Distance from each other. But I have begun to think, whether the same end might not be obtained, tho' there was no Distance of Country between the Observations made. But in order to explain my Thoughts the more easily,

easily, I shall begin with mentioning what the learned *Huygens* has laid down, in his *Dissertation on the Cause of Gravity*, when he endeavoured to discover how much a Pendulum ought to be shortened, which is carried from *France* to the Equator. But as his Figure is so constructed, that all the Lines seem to be in the same Plane, I have endeavoured to form a new Scheme to represent scenographically a Part of an armillary Sphere, which will help the Imagination better, and at the same Time be more convenient for the Addition of those Parts, which serve to explain what I propose.

annes Marchio Poleni, *F. R. S.* No. 468. p. 299. Jan. 1742-3. Fig. 93.

The Circle P A Q E (says *Huygens*) *represents the Earth, cut by a Plane passing thro' each Pole* P Q (therefore this Circle will be a Meridian). *The Centre is* C: *the Equinoctial Circle* E F A G: *The Parallel of Paris* D N O: *Paris* D: K H *represents a Rope sustaining a leaden Weight* H: *which recedes from the Perpendicular* K D C, *because it is thrown back by the circular Motion, according to the Line* D M, *which I suppose to pass thro' the Weight* H. Now D M is a Tangent to the Circle D N O, the Parallel of *Paris*, in the Point D.

Now if we would know what should be the Situation of the Thread K H, *and how much less the Lead* H *should gravitate, than if it hung perpendicularly according to* K D, *we must consider the Point* H, *as if drawn by 3 Threads* H C, H M, H K; *of which* H C *draws toward the Centre of the Earth with the whole Weight which the Plummet would have, if the Earth stood still:* H M *draws according to it's proper Direction, with the (centrifugal) Force given by the Motion of the Earth, in the Circle* D N O: *and* H K *is drawn, or draws, with that Force which is sought. Therefore if* C H *be produced, and* K L *drawn parallel to* D M, *it is known that the 3 Sides of the Triangle* H L K *are proportional to the Powers which draw the Point* H; *and that the Side* L H *answers that which draws by* H C; *the Side* K L *to that which draws by* H M; *and the Side* H K *to that which draws or sustains the Plummet by the Thread* K H. *But the Triangle* K D H *is imagined to have it's Sides equal to the Sides of the Triangle* H L K; *because* C H L *is as it were parallel to* C D K. *Therefore the Sides of the Triangle* K D H *answer to the same Powers: namely, the Side* K D *to the absolute Gravity of the Weight* H, *which it would have, if the Earth stood immoveable*; D H *to the Power which the daily Motion* (producing the centrifugal Force by the Tangent D M) *gives it*; *and* K H *to the Gravity sought.* But I consider the *Power* of the centrifugal Force, namely that which answers to the Tangent D H.

Thus far I have laid down from *Huygens*'s Method what greatly related to my Purpose; but so far only as *is necessary to consider* the Plummet H, *as drawn by the 3 Threads* H C, H M, H K; when the Plummet H is held immoveable by these 3 Threads, or by these 3 Powers. But if it must be moved; that is, if the Pendulum oscillates; I suspect that new Considerations must be had of that Motion of Oscillation: Therefore I shall make a step towards them, and now treat of the Parts, which must be added to the Figure.

But

But before I ſpeak of theſe, I ſhall obſerve, that I have made uſe of a Figure accurately formed of ſolid Parts of a thicker iron Wire. I ſhall alſo obſerve (conſidering the *Hypotheſis* of the Earth's being moved) that one and the ſame Arch is not perfectly deſcribed in the ſame Plane, in one Oſcillation of the *Pendulum*, from it's Centre ; and at the ſame time, as the differences thence ariſing do not diſturb my Purpoſe, that I may ſafely neglect them,

With regard to my Figure, I deſire it may be perfectly underſtood, that thro' the Point H a Plane is drawn parallel to the Meridian P A Q E, and that in this Plane an Arch is marked B T V ; which, when the Pendulum K H oſcillates in ſuch a manner, that the Centre of the Plummet H never departs from that Plane, would be deſcribed by the ſame Centre in the ſame Plane. Let this Arch B T V be called the firſt Arch.

Then imagine another Plane to be extended thro' the Tangent D M and the *Radius* D C, and the Arch R I S to be delineated on this Plane, which, when the Pendulum K H oſcillates in ſuch a manner, that the Centre of the Plummet H never departs from this Plane, would be deſcribed from the ſame Centre in this Plane. Now it is manifeſt, that thoſe 2 Arches B T V, R I S interſect each other in right Angles at H.

Theſe things being thus laid down, two Caſes worthy of particular Attention occur ; or 2 Directions of oſcillating Pendulums are chiefly to be conſidered : one by the firſt Arch B T V, the other by the ſecond R I S.

As to the firſt ; when the Pendulum oſcillating by the firſt Arch BTV, is moved in a Plane which is always equidiſtant, by the Space of the Length of the Line D H, or is always ſo much diſtant as the value of the whole centrifugal Force by the Tangent D H ; it ſeems to be clear in this caſe, that the Power of the centrifugal Force by D H, the Power of the Gravity by H C, and the Power of the Thread by H K, notwithſtanding the Oſcillation of the Pendulum, are always tempered together by the ſame Proportion, which *Huygens* has explained, ſerving alſo to keep the Pendulum immoveable, as I mentioned before.

As to the other ; in which the Pendulum is moved by the ſecond Arch R I S in the ſame Plane, in which the Line of Direction of the centrifugal Force is D M. In this Caſe that Force does not ſeem to act, ſo as to endeavour to draw the Centre of the Plummet H from this it's Plane ; but whilſt the Pendulum tends from R to S, this Force ſeems alſo (as it acts in the ſame Plane by it's Direction from D to M) to concur in increaſing the Motion of the Pendulum. But, on the contrary, whilſt the Pendulum retires from S to R, that Force ſeems, by it's Direction from D to M, to retard the Motion of the Pendulum.

Therefore the proper Motion of the Pendulum, or that which would be referred to one central Gravity acting according to D C, in the firſt Caſe

Cafe of the Excurfion by the Arch B T V, is varied by the centrifugal Force, becaufe it is affected by the Motion arifing thro' D H, from that centrifugal Force, with which Motion it muft neceffarily be compounded. But in the fecond Cafe of the Excurfion by the Arch R I S, it is varied, becaufe in one entire Excurfion toward S, it is accelerated by the fame Force, directed from D to H; but it is retarded alfo by the fame Force in the contrary return towards the oppofite Side R.

Therefore as it feems confonant both to Reafon and Calculation, that the Variation made in the Arch B T V is not equalled by that made in the Arch R I S; it alfo becomes probable, that there muft be fome difference between thofe 2 Cafes, namely, between the Motions of an ofcillating Pendulum according to the fecond Arch R I S, and thofe according to the firft Arch B T V.

Thefe few things being now propofed, I have fufficiently fhewn what I think I have difcovered, by the Obfervation of that difference. For I think I have difcovered a Method of finding, by the help of Obfervations, fomething about the centrifugal Force, which has been applied to the Rotation of the Earth about it's own *Axis*, tho' no Alteration of Place is made between the making of the Obfervations.

To the Cafe of the firft Arch will anfwer a Pendulum placed upon any Meridian Line, fo that the Ofcillations may be made as near as poffible according to that Line: And to the Cafe of the fecond Arch a Pendulum will be accommodated, if it is fo placed, that the Line of the Ofcillations is at right Angles with the Meridian Line. We might have longer Pendulums to our Clocks for fuch Experiments, namely, of the Length of 9 horary Feet.

The Report of the Committee of the Royal Society *appointed to examine fome Queftions in Gunnery.* No. 465. p. 172. *Read* Nov. 4. 1742.

VI. Dr *Jurin* having propofed * two Queftions in Gunnery to be examined, the *Society* was pleafed to appoint a Committee for that Purpofe.

The Queftions were,

1. *Whether all the Powder of the Charge be fired, before the Bullet is fenfibly moved from it's Place?*

2. *Whether the Diftance to which the Bullet is thrown, may not become greater or lefs, by changing the Form of the Chamber, though the Charge of Powder and all other Circumftances continue unchanged?*

At the Meeting of the Committee it was propofed to divide the Firft Queftion into two Parts.

1. *Whether all the Powder of the Charge be fired?*

2. *Whether all the Powder that is fired, be fired before the Bullet is fenfibly moved from it's Place?*

As to the Firft part of the Firft Queftion, *the Committee are of Opinion, that all the Powder of the Charge is not fired.*

They found their Opinion upon the following Experiments:

* *June* 24. 1742.

Pieces

Pieces of Paper ufed for Hangings were laid clofe together upon the Ground, to the Breadth of ten Feet, in the Line of a Fowling-piece, between it and a Frame of 10 Feet fquare, covered over with Paper. Upon pointing the Piece towards the Middle of the Frame, and *difcharging* it feveral times *with* and *without Ball*, fome Powder was always collected, but mixed with a great deal of Dirt.

It is however to be obferved, that in two Experiments made the 22d of *July*, near the Artillery-Ground, before the Prefident and fome of the Fellows of the *Society*, with a finer fort of Powder, in a Barrel of 3 Feet 9 Inches in Length, and $\frac{3}{4}$ of an Inch Bore, with 12 *dwt.* of Powder the firft time, and 24 *dwt.* the fecond Time, without Ball or Wadding, no Powder could be found fcattered on the Paper laid before the Piece, nor fticking to a Board at the Diftance of about 10 Feet, againft which the Piece was pointed. But when the fame Powder was fired in a fhort Barrel of $5\frac{2}{10}$ Inches in the Chace, either with or without Ball, fome Quantity of Powder was always collected.

Other Experiments were afterwards made before the Committee, by firing a Fowling-piece charged with 5 *dwt.* of Powder, againft a Sheet of whited-brown Paper, at the Diftance of 2 or 3 Yards; the Paper was found pierced with feveral Hundred Holes, and the Jags of the Paper appeared on the Backfide. In a fecond Trial with 10 *dwt.* the Paper had more Holes in it. A third Trial was made with 5 *dwt.* of Powder and Ball, and then few Holes appeared in the Paper. In a fourth Experiment made with a fhort Screw-barrel Piftol, with a Charge of 1 *dwt.* 2 Grains of Powder and a Ball, feveral Holes were found in the Paper *.

But the Irregularities in this manner of collecting the Powder unfired, giving reafon to fufpect, that fome Powder efcaped fideways, beyond the Paper laid to receive it, it was propofed to have a Machine made, which being clofe every where but at the End where the Muzzle of the Piece was to be placed, might thereby hinder the Powder from being diffipated. Such a Machine was contrived by Mr *Ellicot*, and by him prefented to the Committee, being a Frame of Wood in Shape like a truncated quadrangular Pyramid; at the fmaller End was a Board to receive the Shot, and the 4 Sides of the Machine were covered with thick Paper ftrongly pafted together, and fo prepared as to prevent it's taking Fire. This Machine, fupported by Props, was placed upon one of it's Angles, the Carriage for fixing the Barrels was placed clofe to the greater Bafe, which was left open. The Refult of the feveral Experiments were as follows:

The 3 firft Experiments were made with a Barrel $\frac{8}{10}$ of an Inch Diameter of the Bore, and the Length of the Chace $5\frac{2}{10}$ Inches. The Charge

* That the Paper in thefe Experiments was pierced by the unfired Powder, appears, becaufe feveral Grains were found lying behind the Frame, to which the Paper was fixed, and fome few ftuck in the Paper.

each time was 6 *dwt.* of Powder without Ball; the Quantities of Powder collected were refpectively, 1 *dwt.* 19 Grains; 1 *dwt.* 21 Grains; and 1 *dwt.* 20 Grains.

Three other Experiments were made with the fame Piece, and with 12 *dwt.* Charge, without Ball. The Quantities of Powder collected were 4 *dwt.* 18 Grains; 4 *dwt.* 2 ½ Grains; and 4 *dwt.* 22 Grains.

The next 3 Trials were with the fame Piece, the Charge 6 *dwt.* with a Ball weighing one Ounce 4 *dwt.* being a Mixture of Lead and Tin, and fitting the Piece exactly.

The Quantities of Powder collected each time were refpectively 1 *dwt.* 5 Grains; 1 *dwt.* 5 Grains; and 1 *dwt.* 11 Grains.

The laft 3 Experiments with the fame Piece, were made with a Charge of 12 *dwt.* the Weight of the Ball as before; and the Quantities of Powder collected, were found to be 1 *dwt.* 12 Grains; 1 *dwt.* 9 Grains; and 1 *dwt.* 8 ½ Grains.

The Waddings ufed in all thefe and the following Experiments, were of thick Leather cut round, to fit the Bore of the Piece.

The Committee then proceeded to examine what Alteration might arife from a greater *Length of Chace.* The Experiments in this Cafe were made with a Barrel 3 Foot 9 Inches in Length, and ¾ of an Inch in the Bore; the Charges of Powder, and Weight of leaden Balls, were as before.

In the firft 3 Experiments with 6 *dwt.* Charge, without Ball, the Quantities of Powder collected were 3 Grains; 9 Grains; and 9 Grains, refpectively. In the 3 next Experiments, with twelve *dwt.* Charge, without Ball, the Quantities of Powder collected were 13 Grains; 9 Grains; and 16 ½ Grains. The 3 following Experiments were with 6 *dwt.* Charge and a Ball. The Powder collected was 2 Grains; 3 Grains; and 2 Grains.

The laft Experiments were made with 12 *dwt.* Charge and Ball as before; the Quantities of Powder collected from 2 Difcharges were refpectively, 2 Grains; and 4 ½ Grains. The Frame being broke, a third Experiment could not be made.

The Powder collected after the feveral Difcharges, was put into feparate Boxes; it feemed much bruifed, and mixed with Dirt. Yet feveral of the Parcels being tried, fired with brifk Explofions; and fome of the Powder collected from the Experiments with the fhort Barrel, amounting to 6 *dwt.* 16 Grains, being put into the long Barrel, and fired with Ball, went off with a ftrong Report; and the Ball pierced the Deal-board at the End of the Frame, and penetrated 2 Inches deep into an Elm-plank placed to receive the Balls.

Some Gentlemen, prefent at thefe Experiments, fufpecting that Part of the Powder might efcape at the open End of the Frame; the fhort Barrel was fired with 12 *dwt.* of Powder and Ball, as before; through a very large Funnel, the Quantities found, after three Difcharges, were feverally, 1 *dwt.* 2 Grains; 16 Grains; and 15 Grains.

Whereas

Whereas upon removing the Funnel, and discharging the Piece, as before, 1 *dwt.* 11 Grains was collected, agreeably to former Experiments; it seems that the Funnel had a like Effect as lengthening the Piece.

Some Experiments were also made with the short Barrel, filled up with Lead, so as to leave but $3\frac{1}{4}$ Inches for the Chace, the Piece being then charged with 12 *dwt.* of Powder and Ball, as before; the Surface of the Ball was but $\frac{8}{10}$ of an Inch within the Mouth of the Piece, and the Powder collected, after 3 Discharges, was respectively, 2 *dwt.* 2 Grains; 1 *dwt.* 17 Grains; and 1 *dwt.* 11 Grains.

The Barrel being further filled up, so as to leave but $2\frac{8}{10}$ Inches for the Chace, and charged as before, the Ball rising about $\frac{1}{7}$ of an Inch beyond the Mouth of the Piece, the Powder collected, after the Discharge, was 2 *dwt.* 6 Grains. Upon a second Trial, the Ball being as much within the Mouth, 1 *dwt.* 16 Grains was collected. And at the third Trial, the Ball being level with the Mouth, 2 *dwt.* 6 Grains were again found.

The Committee also caused some Experiments to be made of the Effect of a *Touch-hole* near the Forepart of the Charge. They found upon discharging the short Piece of $5\frac{2}{10}$ of an Inch Chace, the Charge 12 *dwt.* and Ball, as before, the Touch-hole being near the Fore-part of the Powder; the Quantities of Powder, severally collected, were 1 *dwt.* $7\frac{1}{2}$ Grains; 1 *dwt.* 6 Grains; and 1 *dwt.* 4 Grains. And upon a Discharge made with a little more Powder, which filled the Barrel exactly to the Edge of another Touch-hole, the former being screwed up, the Quantity collected was 1 *dwt.* 9 Grains.

The Effect of firing with *heavy Slugs* was also examined: The Weight of the Slugs and Quantities of Powder collected, were as follows; the Charge in the short Barrel being 12 *dwt.*

Discharge.	Weight of Slugs.			Powder collected.	
	Ounces.	*dwt.*	*gr.*	*dwt.*	*gr.*
I.	2.	13.	0.	1.	3.
II.	2.	11.	14.	0.	17.
III.	2.	12.	0.	0.	8.
IV.	5.	5.	6.	0.	13.
V.	5.	3.	0.	0.	$8\frac{1}{2}$.

The Powder used in all these Experiments, made before the Committeee, was presented to them by Mr *Walton*, and is such as he makes for the King's Service. To ascertain as nearly as possible, that the Powder had not undergone any considerable Alteration by Damps or otherwise, a Standard Experiment was previously made at every Meeting, with the short Barrel charged with 12 *dwt.* of Powder, and with a Ball of 24 *dwt.*; and the Quantity of Powder collected was from 1 *dwt* 8 Grains, to 1 *dwt.* 12 Grains; which is as great a Regularity as can well

well be expected. This Powder of Mr *Walton*'s being *sifted*, and divided into a *fine* and a *large Sort*, the following Difcharges were made with 12 *dwt.* of each, and Ball as ufual :

Difcharges with fine Powder.	Powder collected. *dwt.*	*gr.*
I. — — — — — — —	1.	4.
II. — — — — — — —	0.	21.
III. — — — — — — —	0.	12.

In this third Experiment the Bullet, not being fo exactly turned as the others, was rammed down with great Force.

Difcharge with large Powder.	Powder collected. *dwt.*	*gr.*
I. — — — — — — — —	1.	11.
II. — — — — — — —	1.	16.
III. — — — — — — —	1.	21.

And the Powder being bruifed in a Mortar, and fifted through a Lawn Sieve, the Charge and Ball being as before, what was collected after 3 Difcharges. was one *dwt.* 10 Grains, 1 *dwt.* 8 Grains, and 17 Grains.

Mr *Watfon* having had two Parcels of Powder delivered to him, the one frefh, and the other collected after Difcharges with Ball, gave an Account of the Quantity of Nitre he had feparated from them, *viz.*

Separated from 9 *dwt.* of frefh Powder — — —	*dwt.*	*gr.*
Nitre — — — —	6.	2.
Refiduum — — —	2.	7.
Lofs — — — —	0.	15.

From 9 *dwt.* of Powder collected after having been difcharged with Ball — — — — — — —	*dwt.*	*gr.*
Nitre — — —	4.	18.
Refiduum — — —	2.	15.
Sand, &c. — — —	0.	11.
Lofs — — — —	1.	14.

Twelve Grains of the Powder gathered and put into feparate Boxes, after firing with Ball out of the fhort Piece, as before-mentioned, being fired in the exhaufted Receiver, funk the Mercurial Gage from 29 $\frac{1}{10}$ Inches to 23 $\frac{6}{10}$. And the fame Weight of frefh Powder being fired in the fame manner, funk the Gage to 22 $\frac{3}{4}$ Inches; the Difference being $\frac{85}{100}$ or $\frac{17}{20}$ of an Inch.

From these Experiments the Committee are of Opinion, that the first Part of the first Question, *Whether all the Powder of the Charge be fired?* is sufficiently determined in the *Negative.*

As to the Second Part of the first Question, *Whether all the Powder that is fired, be fired before the Bullet is sensibly moved from it's Place?* the Committee are of Opinion, *That the Bullet is sensibly moved from it's Place, before all the Powder that is fired, has taken Fire.*

This, indeed, has not been determined by any direct Experiment, but seems a Consequence of the Determination of the first Part of the Question, that the whole of the Charge is not fired.

For let it be considered, that from the Moment any Part of the Powder within the Barrel takes Fire, the Flame of the Powder already fired is always contiguous to some Part of the Powder as yet unfired; and consequently some Part of this last must be continually taking Fire, so long as any unfired Powder remains within the Barrel; that is, the firing of the Powder cannot be over, till all the unfired Powder is driven out of the Gun: But before any Part, how small soever, of the unfired Powder is driven out of the Gun, the Bullet which lies between the Charge and the Muzzle, must necessarily have been driven out of the Gun. Therefore the firing of the Powder is not over, or all the Powder that is fired, is not fired, till after the Bullet is driven out of the Gun. And consequently the Bullet must be sensibly moved from it's Place, before all the Powder that is fired, has taken Fire.

As to the second Question, *Whether the Distance to which the Bullet is thrown, may not become greater or less, by changing the Form of the Chamber, though the Charge of Powder and all other Circumstances continue unchanged?*

The Committee are of Opinion, *That the Change of the Form in the Chamber, will produce a Change of the Distance to which the Bullet is thrown.* Their Opinion is grounded upon the following Experiments, in which the *longest Chamber* of equal Capacity drove the Ball farthest.

Three brass Chambers were made, whose Depths were respectively 3 Inches; 1 ½ Inch; and ¾ of an Inch; so turned as to fit the Chamber of Mr. *Hauksbee*'s Mortar; each of these Chambers contained, when full, 1 Ounce *Troy* of Powder. The Ball was of Brass, weighing 24 Pound, 6 ½ Ounces *Avoirdupois*, that is, nearly 356 Ounces *Troy* *.

The Ball touched the Powder of the Charge in all these Experiments. With the first Chamber of 3 Inches deep, the Elevation of the Mortar being 45 Degrees, the Ranges at 4 different Trials were found to be,

* Supposing 14 Ounces 11 *dwt.* and 15 Grains and an half, *Troy*, equal to 1 Pound *Avoirdupois*.

Shot,

Shot.	Chains.	Links.	
I. — — — — —	11.	39.	or nearly 752 Feet.
II. — — — — —	10.	38.	685.
III. — — — — —	11.	17.	737.
IV. — — — — —	11.	10.	733.

In the Second of thefe Experiments, the brafs Chamber, not being fufficiently thruft home before the Difcharge, was by the Violence of the Powder driven in fo, that it could not be got out again without the Help of an iron Screw, and a vaft Force applied to iron Wedges. This was doubtlefs the Caufe of the great Irregularity obferved in this Cafe. The mean Diftance, collected from the other 3 Experiments, is nearly 741 Feet.

Then 3 Difcharges were made with the Chamber ¾ of an Inch deep, with Ball, Powder, and Elevation, as before. The Ranges were,

Shot.	Chains.	Links.	
I. — — — — — —	7.	6.	or 466 Feet nearly.
II. — — — — —	7.	2.	463.
III. — — — — —	7.	2.	463.

The mean Diftance to which the Ball was thrown in thefe three Experiments is 464 Feet.

The Chamber 1 ½ Inch deep, was alfo tried; but this not fitting the Mortar fo well as the other 2, the Ranges were found to be very irregular, being

Shot.	Chains.	Links.	
I. — — — — —	10.	40.	or nearly 686 Feet.
II. — — — — —	9.	6.	598.
III. — — — — — —	7.	8.	467.

The laft Shot, falling fo much fhort, may be afcribed to the Damp, it being late in the Evening when it was fired.

That Moifture greatly weakens the Effect of Powder, is commonly known; and the Committee found by an Experiment, That Powder dried by means of a Phial in *Balneo*, and put warm into the Chamber, threw the Ball twice as far as the fame Quantity of Powder taken out of the fame Barrel, before it was dried.

An Account of a Book intituled, New Principles of Gunnery, containing the Determination of the Force of Gunpowder;

VII. This Treatife contains 2 Chapters. The Firft treats of the Force of Gunpowder, and the Velocities communicated to Bullets by it's Explofion: The fecond confiders the Refiftance of the Air to Bullets and Shells moving with great Velocities; and endeavours to evince, that this Refiftance is much beyond what it is generally efteemed to be; and confequently that the Tract defcribed by the Flight of thefe Projectiles, is

and an Investigation of the resisting Power of the Air to swift and slow Motions; by B. R. F. R. S. as far as the same relates to the Force of Gunpowder. Read April 14. *and* 21. 1743.

is very different from what is usually supposed by the modern Writers on this Subject.

The principal Points endeavoured to be established in the first Chapter are these, " That the Force of fired Gunpowder is no more than the " Action of a permanent elastic Fluid, which is produced by the Ex- " plosion; that this Fluid observes the same Laws with common Air " in their Exertion of it's Pressure or Elasticity;" and consequently, " That the Velocities communicated to Bullets by the Explosion, may " be easily computed from the common Rules, which are established " for the Determination of the Air's Elasticity."

The two first Propositions contain the Proofs that a permanent elastic Fluid is constantly generated in the Explosion of Gunpowder; this is evinced by well known Experiments daily repeated, and acquiesced in by all who have frequented the usual Courses of Experimental Philosophy, of which these Experiments generally make a Part; so that the Author presumes he may consider this Point as incontestibly established, at least he has never yet met with any who have questioned it.

The third Proposition is, That the Elasticity of this Fluid produced by the firing of Gunpowder, is, *cæteris paribus*, directly as it's Density; and the Experiment by which this was confirmed, was letting fall separately 2 Quantities of Powder, the one double the other, on a red-hot Iron included in an exhausted Receiver; and it appeared by the Descent of the Mercury, that the Elasticity of the Fluid produced from the double Quantity of Powder, was nearly double the Elasticity of that produced from the single Quantity; that is, the Elasticity was nearly as the Density of the Fluid.

But it may perhaps be thought, that a single Experiment is too slender a Foundation on which to build so material a Principle, since all subsequent Reasonings on the Force of Powder in some measure depend on it. In Reply to this it may be said, that the Author recited this single Experiment on account of the great Quantity of Powder made use of in it, which was $\frac{3}{16}$ of an Ounce; but that he had really made many more equally conclusive, which he thought it unnecessary to mention. However, those who doubt of this Proposition, may satisfy themselves herein by some Experiments made by the late Mr *Hauksbee* before this *Society*, though with a different View; where, by the firing of 26 Quantities of Powder successively, the mercurial Gage was sunk from 29 $\frac{1}{2}$ Inches, to 12 $\frac{1}{4}$; for by comparing these Experiments together, and making the necessary Allowances, it will be found, that the Elasticity was nearly proportional to the Density in all that Variety of Densities.

In this Proposition, the Analogy between the Fluid produced by the Explosion of Powder and common Air, is established thus far, that they exert equal Elasticities in like Circumstances; for this Variation of the Elasticity, in proportion to the Density, is a well known Property of common Air. But other Authors, who, since the Time of Mr *Boyle*, have

have examined the factitious elastic Fluids produced by Burning, Distillation, &c. have carried this Analogy much farther, and have supposed these Fluids to be real Air, endued with all the Properties of that we breathe; particularly the Reverend Dr *Hales*, who has pursued this Examination with the greatest Exactness, in a Series of the best contrived Processes, constantly affixes the Denomination of Air to these factitious Fluids, he having found, that their Weight is the same with that of common Air, and that they dilate with Heat, and contract with Cold; and that they vary their Densities under different Degrees of Impression in the same Proportion with common Air; and from hence, and other Circumstances of Agreement between them, he supposes them to be of the same Nature with Air, and conceives them to be fitly designed by the same Name.

But so perfect a Congruity between these factitious Fluids and Air is not necessary for the Purposes of this Treatise. The fundamental Positions of this first Chapter supposing no more, than that the Elasticity of the Fluid produced in the Explosion of Gunpowder is always, *cæteris paribus*, as it's Density; and that the Force of fired Gunpowder is only the Action of that Fluid modified according to this Law. It has been already mentioned, on what Grounds the First of these Principles hath been asserted, as contained in the Third Proposition; and it remains to explain the Reasons urged for the Support of the last in the 8 succeeding Propositions.

The Law of the Action of this Fluid being determined, 2 Methods offer themselves for investigating the absolute Force of Powder on the Bodies it impels before it. The first by examining the Quantity of this Fluid produced by a given Quantity of Powder, and thence finding it's Elasticity at the Instant of the Explosion; the other by determining the actual Velocities communicated to Bullets by known Charges, acting through Barrels of different Dimensions. The first is the most easy and obvious, but the second the most accurate Method; and therefore the Author has separately pursued each, and he has found, that their Concurrence has greatly exceeded his Expectation, and thereby both of them receive an additional Confirmation.

The Quantity of the elastic Fluid, produced by the Firing of a given Quantity of Powder, is determined by firing it in an exhausted Receiver, and observing how much the mercurial Gage subsides thereby, making a proper Allowance for the Increase of it's Elasticity from the Heat of the included hot Iron. But then as the Subsiding of the Mercury is not measured till the Flame of the Powder is extinguished, and the Fluid is reduced somewhat near the Temperature of the external Air, it is evident, that the Elasticity thus estimated is much short of what it really was in the Instant of Explosion; and therefore, to obtain that Elasticity, which is the Force sought, it is necessary to make some Estimate of the Increase of the Elasticity of the Fluid by the Fire and Flame of the Explosion. For this Purpose it is examined in the Fifth

Propoſition, how much the Elaſticity of common Air is increaſed by a Degree of Heat equal to that of Iron beginning to grow white hot; and it is found, at a Medium, to be thereby augmented ſomething more than 4 Times; whence, as the Fluid produced by any Quantity of Gunpowder takes up, when compreſſed by the Weight of the incumbent Atmoſphere, a Space ſomething leſs than 250 times the Bulk of the Powder; it follows, that if it's Elaſticity in the Inſtant of Exploſion be ſuppoſed to be increaſed in the ſame Proportion with that of the Air laſt-mentioned, it becomes by this means about 1000 times greater than the Preſſure of the Atmoſphere; that is, conceiving it to be contained in that Space only which the Powder occupied before it was fired.

Thoſe who have not been converſant in theſe Experiments, may poſſibly ſuppoſe, that the Elaſticity of the Powder at the Inſtant of Exploſion may be immediately known by the firſt ſudden Deſcent of the Mercury: But many Circumſtances concur to render this Method impracticable; amongſt the reſt it muſt be remembred, that ſome Air is conſtantly left in the Receiver, which is heated by the Blaſt, and unites it's Effects in the firſt Inſtant with the Action of the Powder: Beſides, the firſt Deſcent may be varied, by varying the Tube, although all things elſe remain unchanged.

By the Method hitherto deſcribed, it is collected, that the Elaſticity of the Fluid produced from fired Gunpowder, when contained in the Space which was taken up by the Powder before the Exploſion, is about 1000 times greater than the Elaſticity of common Air, or, which is the ſame thing, 1000 times greater than the Preſſure of the Atmoſphere.

But, beſides the Determination of the Quantity of Fluid produced from a given Quantity of Powder, (the Method on which this Deduction is founded) there is another Method of diſcovering the ſame thing, which, though leſs obvious, is yet (as hath been already obſerved) more accurate: That is, by examining the actual Velocities communicated to Bullets by the Exploſion of given Charges in given Cylinders; and this is the Subject of the 7th, 8th, and 9th Propoſitions.

And firſt, it is evident, that this Examination cannot take place, unleſs a Method of diſcovering the Velocities of Bullets be previouſly eſtabliſhed. Now the only known Means of effecting this was, either by obſerving the Time of the Flight of Bullets through a given Space; or by finding their Ranges when they were projected at a given Angle, and thence computing their Velocity on the Hypotheſis of their parabolic Motion. The firſt of theſe Methods was often impracticable, and in all great Velocities extremely inaccurate, both on account of the Shortneſs of the Time of their Flight, and the Reſiſtance of the Air. The ſecond is ſtill more exceptionable, ſince, by reaſon of the Air's Reſiſtance, the Velocities thus found may be

lefs in any *Ratio* given, than the real Velocity fought. Now, to avoid thefe Difficulties, the Author has invented a Method of determining the Velocities of Bullets, which may be carried to any required Degree of Exactnefs, and is no-ways liable to the forementioned Exceptions; for, by this Invention, the Velocity of the Bullet is found in any Point of it's Track, independent of the Velocity it had before it arrived at that Point, or of the Velocity it would have after it had paffed it: So that not only the original Velocity, with which it iffues from the Piece, is hence known, but alfo it's Velocity, after it has paffed to any given Diftance; and therefore the Variations of it's Velocity from the Refiftance of the Air may be alfo afcertained with great Facility. The Machine for this Purpofe is defcribed in the 8th Propofition, and the Principle it is founded on is this fimple Axiom of Mechanicks; *That if a Body in Motion ftrikes on another at Reft, and they are not feparated after the Stroke, but move on with one common Motion, then that common Motion is equal to the Motion with which the Firft Body moved before the Stroke*: Whence, if that common Motion and the Maffes of the 2 Bodies are known, the Motion of the Firft Body before the Stroke is thence determined. On this Principle then it follows, that the Velocity of a Bullet may be diminifhed in any given *Ratio*, by it's being made to impinge on a Body of a Weight properly proportioned to it; and hereby the moft violent Motions, which would otherwife efcape our Examination, are eafily determined by thefe retarded Motions, which have a given Relation to them. Hence then, if a heavy Body greatly exceeding the Weight of the Bullet, whofe Velocity is wanted, be fufpended, fo that it may vibrate freely on an Axis in the manner of a Pendulum, and the Bullet impinges on it when it is at Reft, the Velocity of the Pendulum after the Stroke will be eafily known by the Extent of it's Vibration, and from thence, and the known Relation of the Weight of the Bullet and the Pendulum, and the Pofition of the Axis of Ofcillation, the Velocity with which the Bullet is impinged will be determined, as is largely explained in the 8th Propofition. Where note, that there is a Paragraph by Miftake omitted in that Propofition, which fhould increafe the Velocity there found in the duplicate Proportion of the Diftances of the Points of Ofcillation and Percuffion from the Axis of Sufpenfion; but this only affects that particular Number, for it was remembered in the Computations of the fucceeding Experiments, the Numbers of which are truly ftated.

It being explained how the Velocities of Bullets may de difcovered by Experiment: The next Confideration is, from thofe Velocities to determine the Force which produced them.

And the Author thought, the beft Method of effecting this was by computing what Velocities would arife from the Action of fired Powder, fuppofing it's Force to be rightly affumed by the Procefs in the preceding Part; that is, fuppofing the Elafticity of the Fluid thence

arifing

ariſing to be at firſt 1000 times greater than that of common Air; for then, by comparing the Reſult of theſe Computations with a great Number of different Experiments, it would appear whether that Force was rightly aſſigned; and if not, in what Degree it was to be corrected.

Preparatory to this Computation, the Author aſſumes in his 7th Propoſition theſe Two Principles:

1ſt, That the Action of the Powder on the Bullet ceaſes as ſoon as the Bullet is got out of the Piece.

2dly, That all the Powder of the Charge is fired, and converted into an elaſtic Fluid, before the Bullet is ſenſibly moved from it's Place.

And in the annexed Scholium he has given the Arguments and Experiments which induced him to rely on theſe Poſtulates, all which is neceſſary at preſent to diſcuſs more at large.

If the Force of Gunpowder was ſuppoſed capable of being determined with the ſame Accuracy and Rigour, which takes place in Subjects purely Geometrical, the firſt of theſe Poſtulates would be doubtleſs erroneous, ſince it cannot be queſtioned but the Flame acts in ſome Degree on the Bullet after it is out of the Piece.

But it is well known, that in Experimental Subjects no ſuch Preciſeneſs is attainable; for thoſe verſed in Experiments perpetually find, that either the unavoidable Irregularities of their Materials, or the Variation of ſome unobſerved Circumſtance, occaſion very diſcernible Differences in the Event of ſimilar Trials. Thus the Experiments made uſe of for confirming the Laws of the Colliſion of Bodies, have never been found abſolutely to coincide either with the Theory, or with each other. The ſame is true of the Experiments on the Running and Spouting of Water, and other Fluids, and of the Experiments made by Sir *I. Newton*, for the Confirmation of his Theory of Reſiſtances; in which, though they often differ from each other, and from that Theory by $\frac{1}{20}$, $\frac{1}{10}$, and even ſometimes $\frac{1}{5}$ Part, yet thoſe ſmall Inequalities have never been urged as invalidating his Concluſions, ſince, in Experiments of that Nature, it was rather to be wondered at, that the Difference between the different Trials was ſo ſmall.

And if ſome minute Irregularities are the neceſſary Concomitants of all complicated Experiments, it may be well ſuppoſed, that the Action of ſo furious a Power as that of fired Gunpowder, which viſibly agitates and diſorders all Parts of the Apparatus made uſe of, cannot but be attended with ſenſible Variations; and it in Fact appears, that in the Table of Experiments inſerted in the 9th Propoſition, the Velocities of Bullets fired from the ſame Piece, charged with the ſame Powder, and all Circumſtances as near as poſſible the ſame, do yet differ from each other by $\frac{1}{50}$, $\frac{1}{40}$, and ſometimes more than $\frac{1}{30}$, of the Whole; and yet the Author does not conceive, that theſe ſmall Differences are any Exception to the Concluſiveneſs of his Principles;

but

but he preſumes, that had he pretended, without diſcloſing his Method, to have computed the Force of Powder, and the Velocities of Bullets, in different Circumſtances, to a much leſs Degree of Accuracy than this, he ſhould have been cenſured, as boaſting of what would have been thought impracticable.

If then the Action of the Flame on the Bullet after it is out of the Piece, is ſo ſmall as to produce no greater an Effect than what may be deſtroyed by the inevitable Variations of the Experiments, the neglecting it entirely, and ſuppoſing no ſuch Force to take place, is both a convenient and a reaſonable Procedure: For indeed, without the Aſſumption of Poſtulates of this kind, it were impoſſible to have proceeded one Step in Natural Philoſophy, ſince no Mechanick Problem hath been ever ſolved, in which every real Inequality of the moving Force hath been conſidered.

Now what induced the Author to ſuppoſe, that this Poſtulate (though not rigorouſly true) might be ſafely aſſumed, was the Conſideration of the ſpreading of the Flame by it's own Elaſticity, as ſoon as it eſcapes from the Mouth of the Piece: For by this means he conceived that the Part of it which impinged on the Bullet might be ſafely neglected, although the Impulſe of the entire Flame was a very remarkable Force.

With regard to the Second Poſtulate, "That all the Powder is "fired before the Bullet is ſenſibly moved from it's Place;" it is incumbent on the Author to be ſtill more explicit, as this *Society* did ſome time ſince appoint a Committee for examining this very Poſition, who, after making a great Number of Experiments, have determined, * *That all the Powder is not fired before the Bullet is ſenſibly moved from it's Place*; and they have at the ſame time aſſigned the Quantities remaining unfired under different Circumſtances.

Theſe Determinations of the Committee are moſt true; but the Author muſt obſerve, that from the Experiments recited by them, and the Quantity of unfired Powder, which they collected, it may be concluded, that in a Barrel of a cuſtomary Length, charged with the uſual Quantity of Powder, the Deficiency of Velocity occaſioned by the Powder remaining unfired will be ſcarcely ſenſible; and in the ſhorteſt Barrel ever uſed by the Author, where the Space the Bullet was impelled through was not five Inches, and where of courſe this Deficiency of Velocity ought to be the greateſt, it cannot amount to $\frac{1}{30}$ Part of the Whole; and conſequently this Poſtulate, though not rigorouſly true, may yet be ſafely aſſumed, in the inveſtigating the Effects of Powder. But before this is more particularly examined, it is neceſſary to explain the Opinions, which have formerly taken place on this Subject.

* See the preceding Paper.

Thoſe who have hitherto wrote on the Manner in which Powder takes Fire, have ſuppoſed it to be done by regular Degrees; the firſt Grains firing thoſe contiguous, and they the next ſucceſſively; and it has been generally thought, that a conſiderable Time was employed in theſe various Communications: For Mr *Daniel Bernoulli*, in his excellent *Hydrodynamica*, has concluded from ſome Experiments made at *Petersburgh*, that the greateſt Part of the Charge eſcapes out of the Piece unfired, and that the ſmall Part, which is fired, does not take Fire till it is near the Mouth. Many Theories too have been compoſed on the Time of the Progreſs of the Fire amongſt the Grains, and the different Modifications which the Force of Powder did thence receive; and it has been generally conceived, that the proper Lengths of Pieces were determinable from this Principle; "That they ſhould "be long enough to give Time for all the Powder to fire."

But the Author being ſatisfied, that no ſuch regular and progreſſive Steps could be obſerved in the Exploſion; and having found, that by loading with a greater Weight of Bullet, and thereby almoſt doubling the Time of the Continuance of the Powder in the Barrel, it's Force received but an inconſiderable Augmentation; and finding too, that doubling or trebling the uſual Charge, the Powder thus added always produced a correſpondent Effect in the Velocity of the Bullet; and diſcovering likewiſe in a Piece near 4 Feet in Length, charged with an uſual Charge of Powder, that the Velocity communicated to the Bullet, during the firſt 3 Inches of it's Motion, was full half the Velocity which it acquired in it's whole Paſſage through the Barrel, and that the Elaſticity or Force of the Powder, in the firſt 3 Inches of it's Expanſion, was, at a Medium, near 8 times greater than in the laſt 2 Feet of the Barrel; he concluded from all theſe Circumſtances, that the Time employed by the Powder in taking Fire was not neceſſary to be attended to in theſe Computations; but that the whole Maſs might be ſuppoſed to be kindled, before the Bullet was ſenſibly moved from it's Place.

And the Experiments reported by the Committee are the ſtrongeſt Proofs, (as far as they extend) that Powder is not fired in the progreſſive Manner uſually ſuppoſed; for when the ſhort Barrel was charged with 12 *dwt.* and with 6 *dwt.* reſpectively, the Quantity of Powder which was collected unfired from 12 *dwt.* did not exceed by 3 Grains, at a Medium, what was collected from 6 *dwt.* although the Bullet was a leſs Time in paſſing through the Barrel with 12 *dwt.* than with 6 *dwt.* it having a leſs Way to move; conſequently the Quantity remaining unfired of the 6 *dwt.* did not continue unfired for want of Time, ſince, when the Piece was charged with 12 *dwt.* the additional 6 *dwt.* was conſumed in a ſhorter Time.

And again, when the Barrel was ſo ſhortened, that the Bullet, being placed cloſe to the Wad, lay with it's outer Surface nearly level with the Mouth of the Piece, ſo that it had not more than half an Inch

Inch to move before the Flame would have Liberty to expand itſelf; yet, even in this ſhort Tranſit of the Bullet, only 2 *dwt.* 1 ½ *gr.* was collected unfired, at a Medium; which is about $\frac{1}{6}$ of the whole Charge, or, if properly reduced, not more than $\frac{1}{12}$ of the Charge: An obvious Confutation of the gradual firing of the Powder in it's Paſſage through the Barrel, and an eaſy Proof, how ſmall an Error will be occaſioned by ſuppoſing the whole Charge to fire inſtantaneouſly, ſince the Error in the Velocity of the Bullet, ariſing from a Deficiency of $\frac{1}{12}$ of the Charge, is $\frac{1}{24}$ of that Velocity only.

I ſay, that the $\frac{1}{6}$ of the Charge, which remained unfired, amounts to no more than $\frac{1}{12}$ when it is reduced as it ought. This Reduction is founded on the other Experiments reported by the Committee, and on the Circumſtances of thoſe Trials on which the Author founded the preſent Poſtulate. The Author has ſuppoſed the Powder, on which he reaſons in this Treatiſe, to be of the ſame ſort with that made for the Service of the Government, a Parcel of which he was favoured with by Mr *Walton*. But this he chiefly kept for a Standard, and generally uſed other Powders, which, on Examination, he found to be of equal Force. Theſe Powders were of a very ſmall and even Grain, and the Committee have found, that by ſifting the Government Powder, and making uſe of the ſmaller Grains, the Quantity remaining unfired was leſs, at a Medium, in the *Ratio* of 5 to 3, than when it was uſed without ſifting.

And again, it was found by extracting the Saltpetre from the Powder collected unfired, that there was leſs Saltpetre contained in it than in real Powder, and this nearly in the *Ratio* of 9 to 7: Theſe two Proportions compounded make the Proportion of 15 to 7, and in this Proportion muſt the Quantities of Powder collected unfired be reduced, in order to determine the Quantities of real Powder remaining unfired, in ſimilar Experiments made by the Author.

And from hence it follows, that in the Experiments made with a Barrel of 5 $\frac{1}{3}$ Inches in Length, where the Ball had not 3 Inches to move, and where the Irregularity ariſing from the Powder unfired ought to have been the moſt ſenſible, the Quantity of real Powder collected unfired from a Charge of 12 *dwt.* would have been no more than 16 Grains at a Medium, or $\frac{1}{18}$ of the whole Charge; and it being found by Experiment, that the Velocities of Bullets placed in the ſame Situation vary in the ſubduplicate Proportion of the Charges, the Deficiency of Velocity ariſing from the Loſs of the $\frac{1}{18}$ of the Charge would be about $\frac{1}{36}$ of the whole Velocity only, which, in the preſent Caſe, is not $\frac{2}{10}$ of an Inch in the Chord of the Arch deſcribed by the Pendulum meaſuring the Velocity, and is a leſs Difference than what frequently occurs in the exacteſt Repetition of the ſame Experiments.

Other Circumſtances occur, which reduce the Inequality ariſing from the unfired Powder ſtill lower; but it is thought, that this is fully

 ſufficient

fufficient to juftify the Poftulate in Queftion, efpecially as, in all Cafes of real Ufe, the Length of the Barrel in proportion to the Quantity of the Charge will be much greater than in the prefent Inftance: Whence the Author prefumes, that, in computing the Velocities communicated to Bullets by the Action of Powder, it may be fafely fuppofed, that the whole Charge is fired before the Bullet is fenfibly moved from it's Place; at leaft there is no Foundation, from the Experiments made on this Subject by the Committee, to fufpect that when fmall-grained Powder is made ufe of, any greater Irregularity will arife from the Application of this Suppofition, than what would otherwife take place from the Intervention of unavoidable Accidents.

It has been thought neceffary to difcufs more at large thefe two Poftulates, becaufe the laft of them being almoft in the very Words of one of the Queftions propofed to be examined by the Committee of this *Society*, and having by them been determined in the Negative, thofe who have not attended to this Subject might fuppofe, that thereby the Author's Principles were entirely overturned: Now this would be a great Injuftice to him, fince he has not relied on this Poftulate as rigorouflly true; for he knew, and has himfelf taken notice in the prefent Propofition, that fome of the Powder efcapes unfired; and he has there made fome Conjectures on the Caufe of it; but, without infifting on the Reality of thofe Conjectures, he adds, that, "Be that as it may, the Truth of our "Pofition cannot in general be queftioned."

And though it appears from what has been already faid, that the Experiments recited by the Committee rather confirm than invalidate the general Senfe of that Poftulate; yet it is but Juftice to own, that they are a full Confutation of the Conjectures of the Author in relation to the Caufe why fome Part of the Powder comes out unfired; for the Author has fuppofed, after *Diego Ufano*, that the Part which thus efcaped, was fcattered in the Barrel, and not rammed up with the reft, or elfe that it was of a lefs inflammable Compofition: But the Experiments made on this Occafion entirely deftroy this Suppofition.

As this, or any other Conjecture on the Caufe of this Accident, (for it plainly appears not to be for want of Time only) has nothing to do with the general Reafoning of the prefent Treatife, it is not neceffary to enter into it in this Place; but it may not be improper to mention, that, on computing the Quantities of Powder collected from different Charges, one of the Committee was led to conjecture, that what was thus collected was only Parts of Grains that had been fired, but were extinguifhed by the Blaft before they were entirely confumed. This Conjecture is ftrengthened by the extreme Minutenefs of the Particles of all the Powder which was collected, and from the Deficiency of the Saltpetre found in it on Examination: It may be added too, that the Author, by gradually heating a Parcel of Powder, hath fet it on Fire, and blown it out again, for at leaft a Dozen times fucceffively; and he will undertake to repeat the Experiment at any time, if it fhould be doubted of.

The

The Poftulates hitherto difcuffed are preparatory to the 7th Propofition. That Propofition is employed in computing the Velocity which would be communicated to a Bullet in a given Piece by a given Charge of Powder, on the Principles hitherto laid down, that is, fuppofing the Elafticity of fired Powder to be at firft 1000 times greater than that of common Air.

In the 9th Propofition thefe Computations are compared with a great Number of Experiments, made in Barrels of various Lengths, from 7 Inches to 45 Inches, and with different Quantities of Powder, from 6 *dwt.* to 36; and the Coincidence between the Theory and thefe Experiments is very fingular, and fuch as occurs in but few philofophical Subjects of fo complicated a Nature.

By this Agreement between the Theory and the Experiments, each Part of the Theory is feparately confirmed; for by firing different Quantities of Powder in the fame Piece, and in the fame Cavity, it appears that the Velocities of the Bullet, thence arifing, are extremely near the fubduplicate Proportion of thofe Quantities of Powder, and this independent of the Length of the Piece: Whence it is confirmed, that the Elafticity of fired Powder in various Circumftances, is nearly as it's Denfity; and this does not only fucceed in fmall Quantities of Powder, and in fmall Pieces, but in the largeft likewife, under proper Reftrictions; at leaft there are Experiments which could not be influenced by this Theory, where the Quantities of Powder were above 100 times greater than what are ufed by this Author, and in thefe Trials this Circumftance takes place to fufficient Exactnefs.

It is prefumed then, that by this Theory a near Eftimate may be always made of the Velocities communicated to Shells or Bullets by given Charges of Powder; at leaft thefe Experiments evince how truly the Velocities of fmall Bullets are hereby affigned; and the Author can fhew by the Experiments of others, that in a Shell of 13 Inches Diameter, impelled by a full Charge of Powder, the fame Principle nearly holds: It is true indeed, that when the Charge is much fmaller than the ufual Allotment of Powder, there are fome Irregularities, which are particularly mentioned at the End of Prop. 9. to which Head too, perhaps, muft be referred the Experiments made by the Committee on the Effect of different fmall Chambers; but in the cuftomary Charges, the Velocities of Bullets refulting from all the Experiments hitherto made, are really fuch as the Theory laid down in the preceding Part of this Treatife requires. And it appears, that thefe Velocities are much greater than what they have been hitherto accounted: And there are Reafons from the Theory to believe, that in Cannon-fhot the Velocities may ftill exceed the prefent Computation.

The afcertaining the Force of Powder, and thence the Velocities of Bullets impelled by it's Explofion, and the affigning a Method of truly determining their actual Velocities from Experiments, are Points from whence every neceffary Principle in the Formation or Management of

Artillery may be easily deduced: Considering therefore the infinite Import of a well-ordered Artillery to every State, the Author flatters himself, that whatever Judgment may be formed of his Success in these Enquiries, he will not be denied the Merit of having employed his Thoughts and Industry on a Subject, which, though of a most scientific Nature, and of the greatest Consequence to the Publick, hath been hitherto almost totally neglected; or, at least, so superficially considered, as to be left in a much more imperfect State than many other philosophical Researches.

With regard to the second Chapter of this Treatise, relating to the Resistance of the Air, the Author has in his Preface mentioned his Intention of annexing to it a Series of Experiments, on the real Track of Bullets, as modulated by that Resistance: And therefore, as he proposes to complete those Experiments this Summer, unless unforeseen Accidents prevent him, he chooses to postpone any Account of the Subject of the second Chapter till that time, when he intends to lay the Result of those Experiments before this *Society*, in order that any Exceptions or Difficulties relating to them, may be examined and discussed before they are published to the World.

An Account of an Instrument or Machine for changing the Air of the Room of sick People in a little Time, by either drawing out the foul Air, or forcing in fresh Air; or doing both successively, without opening Doors or Windows. No. 437. p. 41. Apr. &c. 1735. Fig. 94.

VIII. *Fig.* 94* represents a Case D E C B, containing a Wheel of 7 Feet in Diameter, and 1 Foot thick; being a cylindrical Box, divided into 12 Cavities by Partitions directed from the Circumference towards the Centre, but wanting 9 Inches of reaching the Centre, being open towards the Centre, and also towards the Circumference, and only closed at the Circumference by the Case, in which the Wheel turns by means of a Handle fixed to it's Axis A, which Axis turns in two Iron Forks, or half concave Cylinders of Bell-Metal, such as A, fixed to the upright Timber or Standard A E.

From the Middle of the Case on the other Side behind A, there comes out a Trunk or square Pipe, which we call the Sucking-Pipe; which is continued quite to the upper Part of the sick Person's Room, whether it be near or far from the Place where the Machine stands, in an upper or lower Story, above or below the Machine. There is a circular Hole in one of the circular Planes of the Machine of 18 Inches Diameter round the Axis, just where the Pipe is inserted into the Case, whereby the Pipe communicates with all the Cavities; and as the Wheel is turned swiftly round, the Air which comes from the sick Room, is taken in at the Centre of the Wheel, and driven to the Circumference, so as to go out with great Swiftness at the Blowing-Pipe B, fixed to the said Circumference.

As the foul Air is drawn away from the sick Rooms, the Air in the neighbouring Apartments will gradually come into the Room through the smallest Passages: But there is a Contrivance to apply the Pipes

* *The Model of this Machine, made by a Scale of an Inch to a Foot, was shewn the* Royal Society June 13, 1734. *By Dr* J. T. Desaguliers, *F. R. S.*

which go to the ſick Room to the Blowing-Pipe B, while the Sucking-Pipe receives it's Air only from the Room where the Machine ſtands. By this means freſh Air may be driven into the ſick Room after the foul has been drawn out.

This Machine would be of great uſe in all Hoſpitals, and in Priſons: It would alſo ſerve very well to convey warm or cold Air into any diſtant Room; nay, to perfume it inſenſibly, upon occaſion.

Fig. 95. repreſents the Inſide of the Flat of the Wheel which is fartheſt from the Handle, and next to the Sucking-Pipe. Fig. 95.

1, 2, 3, 4. repreſents the Cavity or Hole which receives the Air round the Axis, having about it a circular Plate of Iron to hold all firm; which Plate is made faſt to the Wood and to the Iron Croſs that has the Axis in it.

g g g, denotes, by a pricked Circle, a narrow Ring of thick Blanketting, which (by preſſing againſt the outſide Caſe, whilſt it is fixed to the outſide of the Flat of the Wheel) makes the Paſſage into the Wheel tight.

H H H is another Circle of Blanketting, likewiſe fixed to the outſide of the Wheel, and rubbing againſt the Caſe; that the Air violently driven againſt the inner Circumference of the Caſe, may have no way out, but at the Blowing-Pipe at B.

There is on the outſide of the other Flat of the Wheel, where the Handle is fixed, a Ring of Blanketting, like H H H, oppoſite to it; but none oppoſite to *g g g*, becauſe the Wood there is not open, but comes home cloſe to the Axis.

Fig. 96. gives a vertical Section of the Wheel and Caſe a little forward of the Axis, drawn by a Scale twice as large as that of the other two Figures. Fig. 96.

A *a*, the Axis ſupported by the Irons A, *a*, cylindrically hollowed, except the upper Part, where a Pin keeps in the Axis. B D, the Caſe with the Sucking-Pipe S *a*. E A, the Prop for one End of the Axis. 1, 2, the Opening into the Wheel. *g g*, the Eminence of the Wood to which is fixed the ſmall Ring of Blanketting. The four black Marks, one of which is near H, repreſent the Sections of the two other Rings of Blanketting.

IX. 1. When the Wheel revolves upon it's Axis, which is performed in this Machine every Revolution in about half a Second, the Air may be conſidered as divided into as many concentrical Circumferences as there are Particles of Air contained between the leaſt and the greateſt Circle, conſequently the centrifugal Forces will be as the Radii; that is in an arithmetical Progreſſion.

A Calculation of the Velocity of the Air moved by the new invented Centrifugal Bellows of 7 Feet in Diameter, and 1 *Foot thick within, which a Man can keep in Motion with very little Labour, at the Rate of two Revolutions in one Second. By* J. T. Deſaguliers, *F. R. S. Ibid.* p. 44.

Let

		Feet
Let R =	Radius of the greateſt Circle	3.5
r =	Radius of the leaſt Circle	0.75
m =	Radius of the middle Circle	$2.125 = r + \frac{R-r}{2} = \frac{R+r}{2}$
v =	Velocity or Space deſcribed in 1″ in the middle Circle, upon the Suppoſition that the Wheel revolves 2 Revolutions in 1″.	26.21
S =	Space deſcribed in 1″ by the Action of Gravity.	16.1

$s = \left\{\begin{array}{l}\text{Space that a Particle of Air receding from the Centre would}\\ \text{deſcribe in 1″ by the Action of the centrifugal Force at the}\\ \text{Circumference of the middle Circle.}\end{array}\right.$

$2\,m : v :: v : s$; therefore $\frac{v\,v}{2\,m} = s$, by *Huygens*'s Rule. Let G and c, expreſs the Force of Gravity, and the centrifugal Force at the middle Circle. Since the Spaces deſcribed in the ſame Time by the Action of 2 Forces are as thoſe Force $S : s :: G : c$, and $\frac{s\,G}{S} = c$, and ſubſtituting in this Expreſſion $\frac{v\,v}{2\,m}$ inſtead of s, we have $\frac{v\,v\,G}{2\,m\,S} = c$; and putting $\frac{R+r}{2}$, inſtead of it's equal m, $\frac{v\,v\,G}{\overline{R+r} \times S} = c$. So that the Ratio of Gravity to the centrifugal Force, at the middle Circle, is that of G to $\frac{v\,v\,G}{\overline{R+r} \times S}$ or that of 1 to $\frac{v\,v}{\overline{R+r} \times S}$; which being multiplied by the Number of the revolving Circles $R - r$, gives for the Preſſure of the Column of Air $R - r$ proceeding from Gravity $\overline{R-r}$, and the Preſſure proceeding from the centrifical Forces $\frac{\overline{R-r} \times v\,v}{\overline{R+r} \times S}$, wherein $R - r$ being a Factor common to both, may be thrown out of the Expreſſion: And ſince the Velocities produced from different Preſſures are as the ſquare Roots of the Preſſures, the Velocity Gravity would give from the natural Weight or Preſſure of $R - r$ will be to the Velocity, the ſame Column would have from the Preſſure occaſioned by the centrifugal Force, as $\sqrt{1}$, or 1 to $\sqrt{\frac{v\,v}{\overline{R+r} \times S}}$.

Laſtly,

Lastly, Since the Velocity proceeding from the Action of Gravity upon a Column $= R - r$, is always a known Quantity; it may be called $= a$ (equal in this Case to 15.38 Ft. *per* Second) and consequently the Velocity proceeding from the centrifugal Force will be $a \times \sqrt{\frac{v\,v}{R + r \times S}}$

or, $a\,v \times \sqrt{\frac{1}{R + r \times S}}$ or $\frac{a\,v}{\sqrt{R + r \times S}}$: That is, in this Machine $\frac{15.38 \times 26.71}{\sqrt{4.25 \times 16.1}} = 49.67$ Ft. *per* Second. And if we add to this the Velocity of the outer Circle in the Tangent of which the Air escapes, which (in the Supposition we made of 2 Revolutions in 1″) is 44 Feet *per* Second, we shall have = 93.67 Feet *per* Second.

N. B. This Calculation suppposes the Bore of the Sucking-Pipe sufficiently great to furnish as much Air as would escape, according to this Velocity; but in this Machine the Sucking-Pipe being no greater than the Ajutage or Blowing-Pipe, the Velocity proceeding from the Pressure occasioned by the centrifugal Force, and from the Velocity in the Tangent (which may be represented by a Column of Air of sufficient Height to give the Velocity of 93.67 Ft. which is 145.882 Ft.) must be divided into 2 equal Parts, one half employed in sucking, and the other in blowing; therefore the Half of 145.882 Feet, which is 72.941 Feet, will represent the Height of a Column of Air, that would occasion the same Pressure with which the centrifugal Force and the circular Motion act in this Machine; and a Column of this Height producing a Velocity of 68.53 Feet *per* Second. This Number will express the Velocity with which the Air is sucked into the Wheel; and the same Number will also express the Velocity of the Air out of the Blower, proceeding from the centrifugal Force, and the circular Velocity of the outer Circle, which is the real Velocity of the Stream of Air out of the Blower of this Machine, *viz.* 68.53 Feet *per* Second, which is at the Rate of a Mile in about 77″, or about 7 Miles in 9′.

—The Uses of the foregoing Machine, by the same, Ibid. p. 47.

2. I send you a further Account of my centrifugal Wheel, which is now fixed in a Room above the House of Commons, to draw away the hot Steam arising from the Candles, and the Breath of the Company in the House, when it is very full, in warm Weather; as also afterwards to drive in a Stream of fresh Air, to spread uniformly all over the House, by coming in at the Middle of the Cieling.

The Uses of this Machine for sick Rooms, for Prisons, for warming, cooling, or perfuming any Chambers at a distance, were spoken of in the Explanation of the Model I shewed the *Society*. The Machine may also serve in a Man of War, to take away the foul Air between Decks, occasioned by the Number of Men in the Ship, and to give them fresh Air in a few Minutes. In every Part of the Vessel every foul Hole may

be rendered wholeſome, and even the Stench and foul Air from the Surface of the Bulge-Water may be carried off. In regard to Mines, the Machine muſt prove of excellent Uſe; for as the Damps (either fulminating, which, taking Fire, deſtroy the Men and ruin the Works, or arſenical, which kill by their poiſonous Nature) are ſome ſpecifically lighter, and ſome ſpecifically heavier than common Air, this centrifugal Wheel can in a little Time drive down Air through wooden Trunks (or Launders) of 7 Inches bore, in ſuch Quantities into the deepeſt Mines as to cauſe all the light Damp to come out at the Top of the Pit; or, by only altering two Sliders, ſuck away all the heavy poiſonous Damp, whilſt wholeſome Air goes down from above Ground into the Pit, ſo as to fill all the ſubterraneous Caverns with freſh and wholeſome Air.

Likewiſe a great many of the Difficulties which attend the carrying on ſubterraneous Paſſages for the Conveyance of Water from Mines (called Soughs, Adits, or Drifts) may be removed by the Help of this Wheel; for the freſh Air may be driven in a very little Time to the Place where the Men are at work, though at the Diſtance of 2, 3, or 4, Miles, and therefore alſo to any intermediate Space; whereas the Practice now is, either to make a double Drift with Communications between the two for the Circulation of the Air, or to ſink perpendicular Shafts or Pits from the Top of the Hill over the Adit; both which Methods are very expenſive, and (I dare ſay) will, upon Tryal, be out-done by the Application of my Machine.

A Deſcription of a new Invention of Bellows, called Water-Bellows, by Martin Triewald, *F. R. S. Captain of Mechanics, and Military Architect to his Swediſh Majeſty.* No. 448. p. 231. June &c. 1738. *dated Stockholm* May 26. 1736. Fig 97.

X. Theſe *Water-Bellows* A and A, are made of Wood, not unlike the Shape of Diving-Bells, in the Form of a *Conus Truncatus*, and conſequently wider below than at top, where they are furniſhed with cloſe Heads B and B, but at the lower Ends E and E, quite open. At the Heads B and B, are two Valves V and V, which open inwardly, and are made like the Claps of other *Bellows*, with their Hinges, and the Valves themſelves covered with Hatters Felt, and are ſhut by an eaſy Steel Spring, till the Air from above opens the ſame, which happens only when theſe *Bellows* receive their Motion upwards; but are ſhut by means of the Preſſure of the Air within, when they ſink down into the Water. On the very ſame Heads are two pliable Leathern Tubes R and R, fixed one at the Top of each *Water-Bellows*, which Tubes are made and prepared in the ſame manner as thoſe uſed in Water-Engines for extinguiſhing of Fire. Theſe Leathern Tubes or Pipes reach from the *Bellows* to Wooden Tubes T, T, which carry the Wind into the Iron Furnace M, or any other Place, according to Pleaſure.

Theſe *Bellows* are likewiſe provided with Iron Chains *k*, K, which are faſtened to two Sweeps S, S, by which means they hang perpendicular from the Beam of the Balance, and at the ſame Diſtance from the Centre of it's Motion C.

On the Balance are two ſloping Gutters F, F, into which the Water alternately runs from the Gutter G, and ſo gives Motion to the whole Work; ſo that theſe laſt-mentioned Gutters F, F, do the ſame Service

as an Over-ſhot, or any other Water-Wheel, and coſt a great deal leſs, but give as even and regular a Motion, as any *Pendulum*, for meaſuring of Time; for as ſoon as ſo much Water runs into either of the aforementioned inclined Plains of the Gutters, ſo that the *Momentum* of the Water exceeds the Friction near the Centre of Motion C, the Gutter immediately moves down with a Velocity increaſing, till the Balance meets with the Reſiſtance of the Wooden Springs H and H, and at the ſame time raiſes the oppoſite *Water-Bellows*, or that *Bellows* which is fixed under the oppoſite Gutter. In the ſame Moment again as the ſaid Gutter begins it's Motion, being come down on the Spring, delivers all the Water it has received; at the very ſame time the Water begins to run into the oppoſite Gutter, which receives it's Load of Water almoſt as ſoon as the former is emptied; ſo that one of the Gutters does it's Effect, as ſoon as the other has done his, and this alternately one after another.

Theſe ſloping Gutters on the Balance do therefore all the Service and Effect which a Water-Wheel does in working the ordinary *Bellows*, and that by means of the Power which the Water applies to the Wheel of giving the ordinary *Bellows* their Motion, after the ſame manner does the Water here empower the ſloping Gutters to do the ſame Work.

But as for the manner and by what means theſe *Water-Bellows* are fit to blow the Fire, and to perform the ſame as Leathern or Wooden *Bellows*, there is no other Reaſon, but the very ſelf-ſame wherein the Effect of the ordinary *Bellows* conſiſts. For an ordinary pair of *Bellows* blow for no other Reaſon, but that the Air, which enters the *Bellows*, and which they contain when raiſed, is again compreſſed or forced into a narrower Space, when the *Bellows* cloſe: Now ſince the Air, like all other Fluids, moves to that Place where it meets with the leaſt Reſiſtance, the Air muſt conſequently go through the Opening which is left for the ſame, with a Velocity proportioned to the Force by which the Air is compreſſed, and muſt of neceſſity blow ſtronger or weaker, in regard to the Velocity by which the Top and Bottom of the *Bellows* meet; the Blaſt alſo will laſt in Proportion to the Quantity of Air that was drawn into the *Bellows* through the Valve or Wind-clap.

This does after the ſame manner happen in our *Water-Bellows*; for the Air, which they contain, cannot force itſelf down through the Water more than through a well-ſecured Deal-board with Pitch; when the *Bellows* are lowered down into the Water, the Air which they contain muſt neceſſarily be compreſſed by the Water, which riſes alternately into the *Bellows* A and A; ſo the Air muſt recede and go through the Leathern Tubes R, R, where the Air meets with the leaſt Reſiſtance. From all which it undoubtedly follows, that the larger, that is to ſay, the more Air theſe *Water-Bellows* are made to contain, and the greater the Velocity is by which they are made to deſcend into the Water, ſo much greater is their Effect; and that the Effect which they are able to

perform, muſt be equal to that of Leathern or Wooden *Bellows* of the ſame Capacity, in containing an equal Quantity of Air.

As to the Advantages which this new Invention has in regard to thoſe uſed hitherto, it is a known Thing, that the Power which works your common *Bellows* uſed at Iron Furnaces, muſt be ſufficient not only to compreſs the *Bellows*, but at the ſame time to force down the Leaver with it's Weight or Counterpoiſe; which Leaver ſerves again to raiſe the *Bellows*, when the Cog or Button on the Axle-tree of the Water-Wheel ſlides off from the *Bellows-tree*, ſo that the Power muſt be ſufficient at once to produce two different Effects; whereas theſe new *Water-Bellows* require ſcarce any greater Power but what is neceſſary to overcome the Friction near the Centre of Motion, or the Axis C; for in this my Invention an Advantage is obtained, which very rarely happens in Mechanicks, viz. *That the Weight to be moved is, as here, on the Balance in Æquilibrio*; ſince the *Bellows* A and A cannot be otherwiſe conceived than as two equal, though heavy, Weights in a pair of Scales, which balance one another, although their Weight be ever ſo great; ſo that, if each of theſe *Bellows* ſhould weigh a Ton, they muſt ſtill equiponderate; which is ſo much eaſier attained to, ſince it requires very little Art to make them both of a Weight, and order them at equal Diſtances from the Centre of Motion. It is conſequently known how ſmall a Power is required to ſet the Scales of a Balance with equal Weights in Motion, notwithſtanding the Weight may be as great as poſſible; all which may with good Reaſon be applied to theſe *Water-Bellows*.

And though it cannot be denied, but that the *Bellows* which ſinks down into the Water-hole or Sump N, grows ſo much lighter, as it loſes of it's Weight in Water, by which means the *Water-Bellows* to be raiſed grows ſo much heavier, as the former loſes of it's Weight by being let down into the Water; yet this is compenſated, if we conſider, that the Water which falls down along the ſloping Gutter, acquires a Power of a falling Body; which Power increaſing in the ſame Proportion as the *Bellows* to be raiſed grows heavier, this Power ſuits admirably well the Weight to be raiſed; for the *Bellows* that ſinks down into the Sump N, does not at once loſe it's Weight in the Water, but gradually as it comes deeper into the ſame; and after the ſame manner the aſcending *Bellows* does not grow at once heavier than the other, but gradually, growing heavieſt juſt when the lowermoſt Edge gets even with the Surface of the Water; and that happens at the ſame Inſtant of Time when the Power of the Water in the ſloping Gutter is at the higheſt pitch, or has received it's greateſt *Momentum*.

This ſhews, I hope, very plain, that the Power required to work theſe *Water-Bellows*, is far leſs, and conſequently leſs Water will be conſumed in working theſe *Bellows* than thoſe commonly uſed; and again, that an Iron Furnace, which for want of Water to work the common *Bellows*, cannot be kept at work longer than 6 Weeks, though

it

it be provided with all other Neceſſaries, may, by means of ſuch *Water-Bellows* as here deſcribed, be kept at work at leaſt as long again.

It is furthermore a known thing to Miners, of what prodigious Loſs and Inconvenience it is, when the Hearth or Mouth of an Iron Furnace is placed low, in a wet and damp Place, which they oftentimes are forced to do, in regard to the Axle-tree of the Water-Wheel which works the *Bellows*; for which Reaſon ſuch Furnaces as ſtand in the like moiſt Places, give daily conſiderably leſs Iron, than others which are better ſituated. There is likewiſe not a ſmall Difficulty to find a fit Stuation for ſuch Iron Furnaces where Iron Guns are caſt, and require deep Pits under the Mouth of the Furnace: But by means of this new Invention of *Bellows*, one may be at Liberty to place the Mouth of the Furnace as high as one pleaſes, ſeeing it is very eaſy to guide the Blaſt by means of Wooden or Leaden Tubes, as far as neceſſary, and in a proper Direction into the Furnace; which Advantage cannot ſo eaſily be obtained by thoſe *Bellows* in common uſe.

Further, this may be accounted as no ſmall Advantage which theſe *Bellows* afford, in being of ſo very eaſy a Structure, that any Carpenter at firſt Sight is able not only to conſtruct the whole Engine, but eaſily repair every Part of the ſame, requiring at the ſame time the leaſt Repairs of any that can be uſed; and if the *Bellows* ſhould be caſt Iron, they would laſt for ſeveral Ages; and when caſt ſtrong, they would not require any Weight to ſink readily in the Water. One might cauſe them to be covered with Lead, or make them of thin Copper with a thick Leaden Hoop at top, to make them ſink. As for their Shape, it is not abſolutely neceſſary they ſhould be of the ſame as the Figure denotes; for in caſe one would not beſtow Iron Hoops on the *Bellows*, they might be made ſquare, in a Triangle, or any other Shape, provided they be as wide again at botttom as at top; and if they be made of Wood, it will be neceſſary to provide an Edge round the Tops, for containing Stones or Leaden Weights, as much as will be found neceſſary to make them ſink readily, when they are lowered down into the Water.

Laſtly, If we will conſider the Charge of thoſe *Bellows* made uſe of at Iron Furnaces, as to the *Bellows* themſelves, the Water-Wheel and it's Axle-tree, *&c.* and compare the ſame with the Coſt of theſe, we ſhall eaſily find a vaſt Difference, not to mention the vaſt Charges of keeping the common *Bellows* in Repair. But before I conclude, I think myſelf obliged to mention, that the Blaſt of theſe *Bellows* is governed and moderated in the ſame manner as the common ones, *viz.* by ſetting more or leſs Water into the ſloping Gutters, and by taking out and letting in Plugs for that purpoſe placed in Holes near the Top of the *Water-Bellows*.

XI. When a long and heavy Body lying on the Ground is to be raiſed up at one End, (like a Leaver of the ſecond Kind) while the other End keeps it's Place and becomes the Centre of it's Motion; the

An Account of ſome new Statical Experiments, by J. T.

Desaguliers, LL.D. F.R.S. No. 445. p. 62. Jan. &c. 1737.

the Prop, that is made use of to support it at any Point in it's whole Length, sustains a certain Pressure from the Beam. Now the Experiments which I shall make are to shew, by a Force drawing always in the Direction of the Prop, what is the Quantity of the Pressure on the Prop, according to the Length of the Prop, the Angle which it makes with the Beam, or with the Horizon, and the Distance from the Centre of Motion of the Beam at which the Prop is applied. For when the Prop is taken away, the Force drawing in the Direction of the Prop will keep the Beam in *Æquilibrio*; and a Force ever so little superior to the Friction added to the Power, will make it overpoise the Beam and raise it higher; but overcome the Power and bring down the Beam, if it be added or applied to the Beam.

Though in every Case and Experiment we have this Analogy taken from mechanical Principles, *viz.* that

The Intensity of the Power :
Is to that of the Weight;
As the Distance of the Line of Direction of the Weight:
Is to the Distance of the Line of Direction of the Power.

Yet to find those Distances nicely in the several Applications of the Prop, we must have Recourse to geometrical Constructions and Reasonings. With these and the algebraical Expressions of the same, the Experiments exactly agree.

I design to give to the Society a Paper upon this Subject, wherein will be explained not only the Investigation of the Proportion between the Power or Pressure sustained by the Prop and the Weight of the Body supported, but also the Determination, of the *Maximums* of Pressure, where there are any, and the Nature and organical Descriptions of some particular Kinds of Curves of the third Order, described by one End of the Prop in it's successive different Situations.

The Numbers made use of in these Experiments are the result of the Calculations; and all I propose now is to shew the Experiments by Means of a Machine which I contrived for the Purpose, and got executed with great Nicety, not in Ornaments, but only where Nicety in a mechanical Instrument ought to be observed; a Caution useful in many other Machines.

In this Machine, the Iron Bar, or Parallellipiped representing the heavy Body, weighs 12 Drams, 12 *dwt*, 12 Grains, or 6060 Grains, and it's Centre of Gravity is at the Distance of 20 Inches and a half from it's Centre of Motion.

The Props I make use of are, the one of five, and the other of ten Inches. To overcome the Friction, allowed for by certain Rules in all Cases, I use a nice Brass Pulley of three Inches Diameter, whose Pivots are but $\frac{75}{1000}$ of an Inch in Diameter; so that the 60th part of the Power added to it, will, in all Cases, overcome the Friction.

FIRST

First Case.

In which the Prop is perpendicular to the Horizon exemplified by two Experiments.

The Prop is equal to 5 Inches, and placed under a Point in the Bar 10 Inches diſtant from the Centre of Motion. Here the Power acting in the Direction of the Prop, able to keep the Bar in that Situation, or the Preſſure ſuſtained by the Prop, will be found 250 Ounces, 17 *dwt.* 15 Grains; and the Friction 8 *dwt.* 15 Grains. The Foot of the Prop is to be at 8 Inches and $\frac{66}{100}$ from the Centre of Motion. Exp. I.

If the ſame Prop of 5 Inches is placed under a Point in the Bar at 30 Inches from the Centre of Motion, the Power or Preſſure will be 8 Ounces, 12 *dwt.* 13 Grains; and the Friction equal to 2 *dwt.* 21 Grains. The Foot of the Prop is to be diſtant from the Centre of Motion 29 Inches $\frac{58}{100}$. Exp. II.

Second Case.

In which the Prop is prependicular to the Bar, exemplified by three Experiments.

Now let the Prop (ſtill five Inches long) be placed ſo as to be perpendicular to the Bar in a Point 12 Inches diſtant from the Centre of Motion. Here the Power expreſſive of the Preſſure ſhould be 19 Ounces, 18 *dwt.* 4 Grains, and the Friction 6 *dwt.* 15 Grains; but on account of a Correction neceſſary to be made to this, (becauſe the Bar is thick as well as heavy, and the Centre of Gravity above the Surface to which the Prop is applied) the Power or Preſſure ſuſtained will be only 19 Ounces, 15 *dwt.* 5 Grains, and the Friction 6 *dwt.* 14 Grains. Exp. I.

N. B. The Diſtance of the Foot of the Prop in this Caſe is 13 Inches from the Centre.

The Prop here is 10 Inches long, (ſtill perpendicular to the Bar) under a Point in the Bar, 24 Inches diſtant from the Centre. The Power equal to the Preſſure ſuſtained ſhould be (if the Bar was only heavy, and not thick) 9 Ounces, 19 *dwt.* 4 Grains; the Friction 3 *dwt.* 11 Grains and an half; but with the proper Correction, which I ſhall explain hereafter, it muſt be only 9 Ounces, 17 *dwt.* 15 Grains; the Friction 7 *dwt.* 7 Grains. Here the Foot of the Prop is to be 26 Inches from the Centre. Exp. II.

If the End of the Prop is placed under a Point in the Bar, ſo that the Horizontal Diſtance of the Foot of the Prop be exactly equal to the Diſtance of the Centre of Gravity from the ſaid Centre of Motion, *viz.* 20,5 Inches; the Power or Preſſure ſuſtained by the Prop will be preciſely equal to the Weight of the Bar, *viz.* 12 Ounces, 12 *dwt.* 12 Exp. III.

Grains. In this Cafe, the Prop is diftant from the Centre of Motion on the Bar 17,9 Inches, and the Friction 4 *dwt.* 5 Grains.

The Third Case.

In which the Angle made by the Prop with the horizontal Line is given, either acute or obtufe.

As this Cafe is very intricate, (on Account of the feveral Powers of the Sine and Cofine of the given Angle, which are multiplied into the Prop and into the Weight of the Beam) we will exemplify it only in one Experiment; which is, when the Angle made by the Prop, with the horizontal Line contained between the Foot of the Prop and the Centre, is acute: Then there is a *Maximum* of Preffure, which I will fhew by Experiment to be the very fame as the Calculation gives. I fuppofe the Angle made by the Prop and the horizontal Line to be 60 Degrees: The Calculation of this *Maximum* fhews, that if the Prop is 10 Inches long, the Diftance meafured upon the Bar, to which the upper End of the Prop muft be applied, will be 10 Inches $\frac{96}{100}$, the Bar itfelf making then an Angle of about 52 Degrees 12 Minutes; and the horizontal Diftance between the Centre of Motion and the Foot of the Prop is then 11 Inches $\frac{72}{100}$.

N. B. Three Things are to be remarked in this Cafe:

1. That when the Angle made by the Prop and the horizontal Line, contained between the Centre of Motion and the Foot of the Prop, is acute, as in the laft Experiment, there is always a *Maximum*: Whereas if the fame Angle was obtufe, there would be no pofitive *Maximum*; for then the Preffure would continually increafe, the nearer the Prop is to the Centre of Motion.

2. That when the Angle of the Prop with the Horizon is acute, as in the laft Experiment, the Bar, or long and heavy Body, can be raifed by applying the Power or Prop always with the fame Angle to the Horizon, quite up to a vertical Situation.

3. That the firft Cafe, which is when the Prop is perpendicular to the Horizon, is only a particular Cafe of this more general one.

The Fourth Case

Is, when the Angle made by the Prop with that part of the Beam contained between the Point to which it is applied, and the Centre of Motion, is given either acute or obtufe.

As the Expreffion of the Power in this Cafe is fully as intricate as in the laft, I will only give one Example or Experiment; and, for the greater Satisfaction of thofe that fee it, I chofe that, wherein the Preffure is in it's *Maximum.* I fuppofe, as before, the Angle made by the Prop, (ftill 10 Inches long) with that Part of the Beam contained

2 between

between the Point to which it is applyed, and the Centre of Motion, to be acute and of 60 Degrees; then the *Maximum* of Preſſure will be, when the part of the Beam intercepted between the Centre of Motion and the upper End of the Prop is 12 Inches $\frac{21}{100}$; the Bar is then elevated about 50 Degrees 13 Minutes, and the horizontal Diſtance between the Centre of Motion and the Foot of the Prop is then 11 Inches $\frac{27}{100}$.

N. B. Obſerve alſo in this Caſe as in the laſt.

1. If the Angle made by the Prop, and the part of the Beam intercepted between the Point of Application and the Centre of Motion, is acute, there will always be a *Maximum.* The contrary will happen, if that Angle is obtuſe.

2. If the Angle is acute, the Bar cannot be raiſed up to a vertical Situation by applying the Power or Prop conſtantly with the ſame acute Angle; but it may be raiſed quite up, if the Angle of the Prop with the Beam is obtuſe.

3. The ſecond Caſe is but a particular Caſe of this general one. For the Reaſons of all thoſe Things, the Corrections neceſſary to be made on account of the Thickneſs of the Bar, the Nature and organical Deſcription of ſome Curves, and ſeveral other remarkable Conſiderations on this Subject, I muſt refer to the Paper I ſhall give in to the Society.

The Figure of a Machine *for grinding* Lenſes *ſpherically, invented by Mr* Samuel Jenkins, No. 459. p. 555. Jan. &c. 1741. *dated* Nov. 29. 1737.

XII. The Advantage it would be to have *Lenſes* of the ſpherical Kind, Segments of a true Sphere, hath occaſioned the Invention of many Machines and Methods of Grinding, in order to produce ſuch Segments: But nothing hitherto made publick hath anſwered the End propoſed.

The beſt Methods now in Uſe will only produce an Approximation to a truly ſpherical Figure, but demonſtrably not one, though the Artificer ſhould employ the utmoſt Skill and Care in the Uſe of the beſt Machines hitherto invented: And indeed, at preſent, Gentlemen have nothing to depend on, that their *Lenſes* are nearly ſpherical, but the Care and Integrity of the Workmen; in which how often they are deceived, is too obvious to every one who hath Occaſion to uſe ſuch *Lenſes*.

I therefore beg Leave to ſubmit to your Conſideration the Effects of a Machine, which, as it is contrived to turn a Sphere at one and the ſame time on two *Axes* which cut each other at Right Angles, with equal Velocity and Preſſure on each of them, I conceive it is demonſtrable, that (without any Skill or Care in the Workman) it will produce a Segment of a true Sphere, barely by turning round the Wheels; which if ſo, the Conſequences will be,

1ſt, That all Grinders of ſuch Glaſſes, *&c.* will gladly uſe them; a labouring Man, whom they hire for leſs Wages, being, by the Help of this Machine, able to do more Work in a Day, that a ſkilful Artificer, without it, in two Days. And,

2*dly*, All Gentlemen will have the Pleafure to know the *Lenfes* they make ufe of are truly fpherical, it being impoffible this Machine fhould produce any other Figure.

Explanation of Fig. 98.

A. *A Globe covered with Cement, in which are fixed the Pieces of Glafs to be ground. This Globe is faftened to the Axis, and turns with the Wheel* B. C. *Is the brafs Cup, which polifhes the Glafs: This is faftened to the Axis, and turns with the Wheel* D. *So that the Motion of this Cup* C *is at Right Angles with the Motion of the Globe* A.

CHAP. VI.

HYDRAULICKS.

Of the Meafure and Motion of running Waters. Effay I. *of Water running out of a Veffel always full thro' a round Hole, and of it's Refiftance arifing from a Defect of Lubricity; by* James Jurin, *M. D. F. R. S.* No. 452. p. 5. Jan. &c. 1739.

I. THE Ancients had no other *Meafure* of running Waters, than that uncertain and fallacious one, which having no Regard to the Velocity, depended wholly on the perpendicular Section of a Stream. The firft, who opened a way to the Truth, was *Benedict Caftelli*, an Italian and Friend of *Galileo*. He having difcovered that the quantity of Water flowing through a given Section of a Stream is not given, as the Ancients thought, but that it is proportional to the Celerity with which the Water is carried thro' the given Section, by this noble Difcovery laid the Foundation of a new and moft ufeful hydraulick Science. This Difcovery therefore engaged the Philofophers to ftudy this Doctrine with fo much Diligence, that after *Caftelli*'s Time there was hardly any eminent Mathematician, who did not endeavour to add fomething thereto, either by Experiments, or by Reafonings and Arguments *à priori*.

But moft of them, notwithftanding their great Abilities, had no Succefs therein, becaufe of the exceeding Difficulty of the Work. For thofe, who ftudied the Theory, laid down fuch Theorems as were found to be falfe, when brought to the Teft by Experiments; and thofe, who laboured in making Experiments, omitting to obferve fome minute Circumftances, the Importance of which they had not yet perceived, differed greatly from one another, and almoft all of them erred from the real *Meafure*.

Of this there cannot be given a better Example than that fimple and eafy one, which has generally been a Foundation to all the reft, and is what we have now undertaken to handle diligently, when Water runs out thro' a circular Hole made in the Bottom of a Veffel conftantly full, with a conftant Velocity. *Poleni* alone has given the true Meafure of the Water flowing out, or at leaft very near the true one; and Sir *I. Newton* alone has laid the Foundation of difcovering that Meafure; tho'

Pla XVII. Vol. VIII. part 1. pa. 282.

Fig. 93.

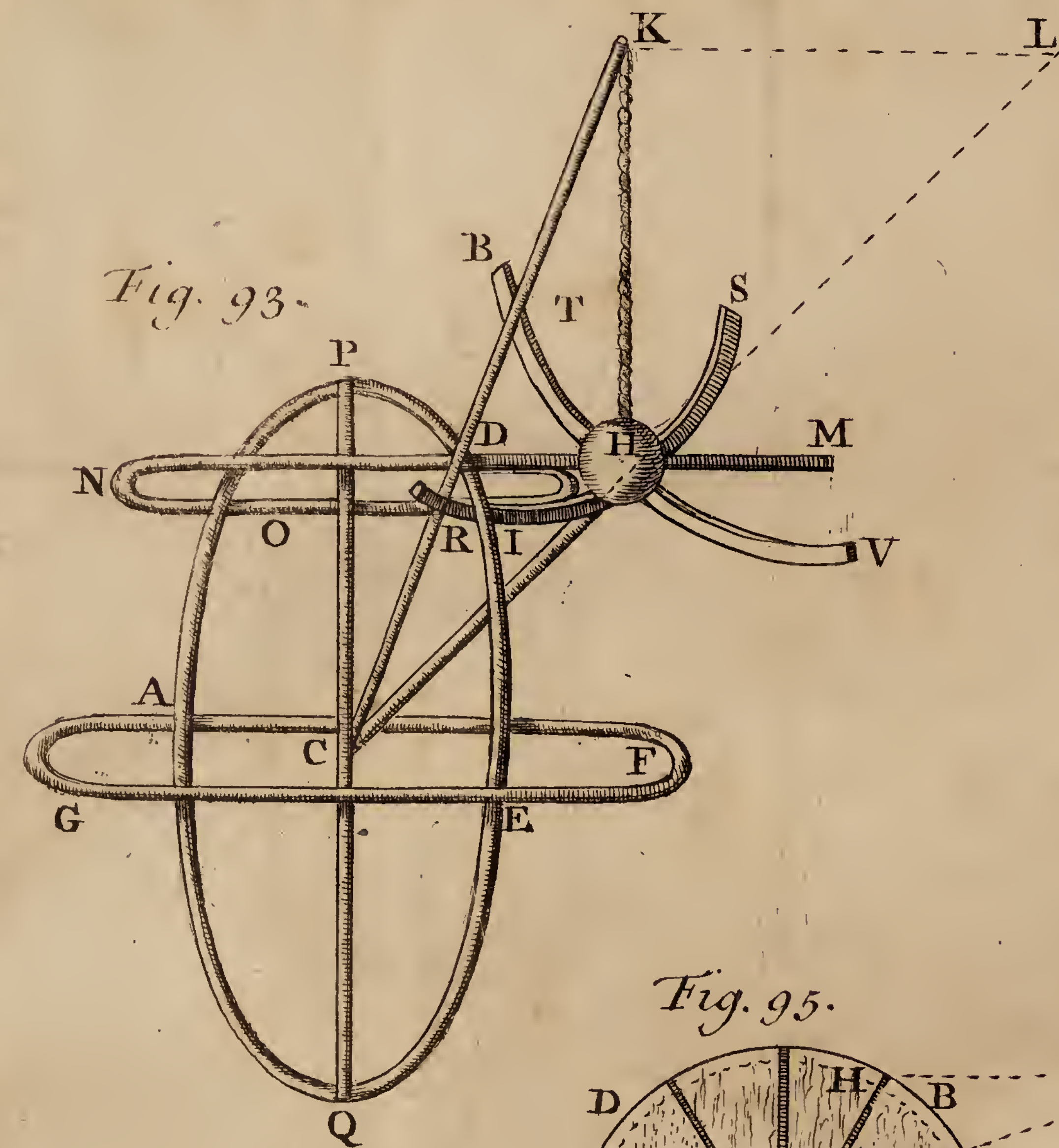


Fig. 97.

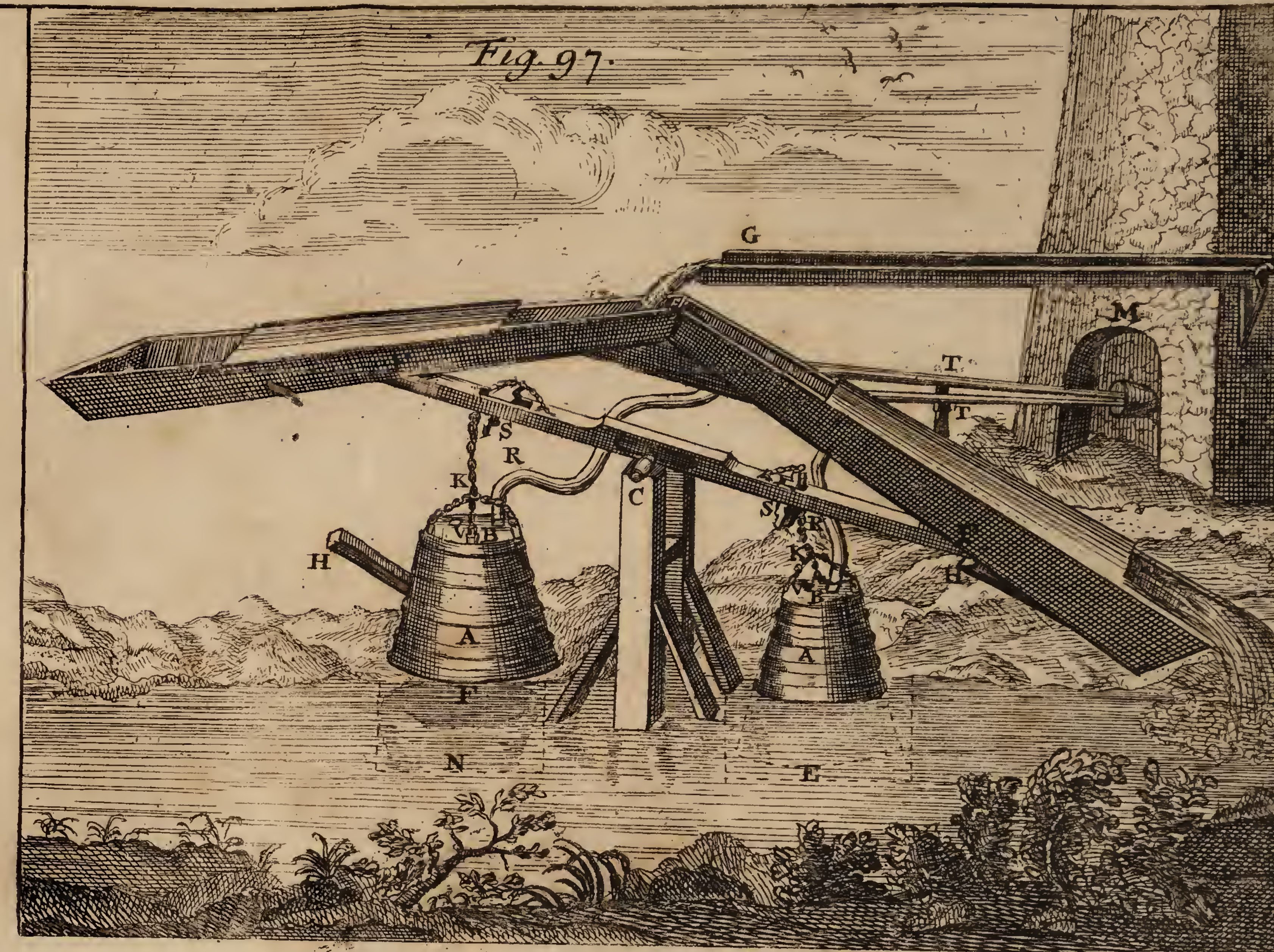


Fig. 95.

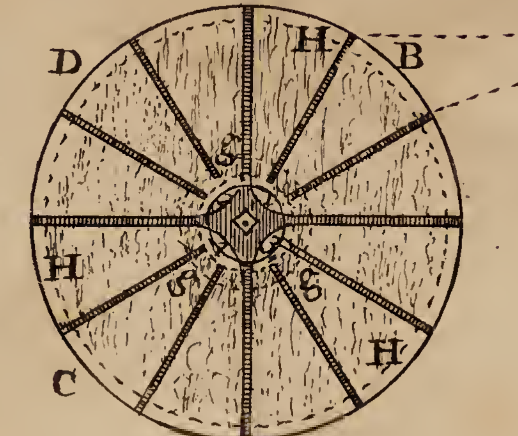


Fig. 96.

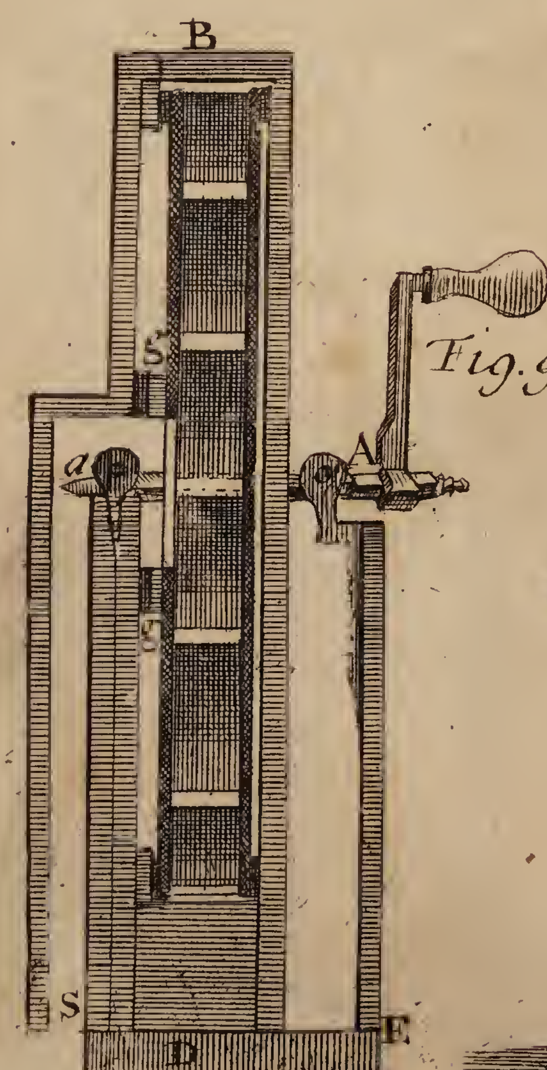


Fig. 94.

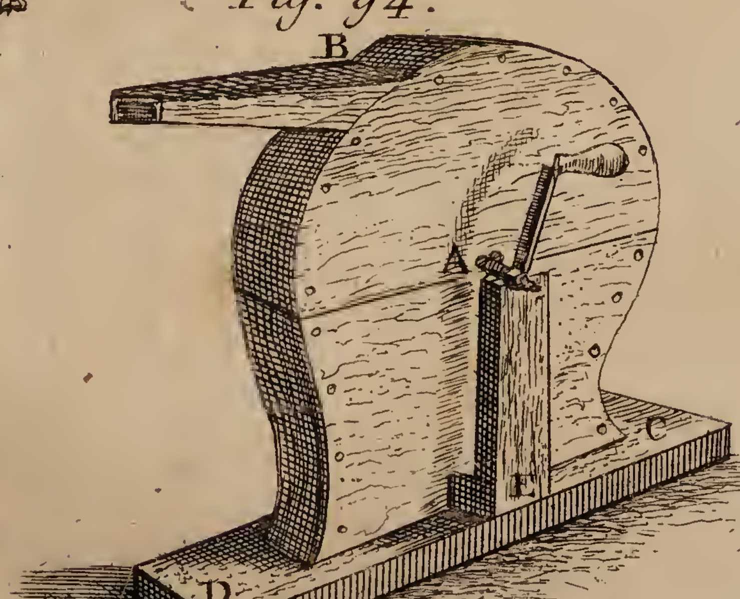
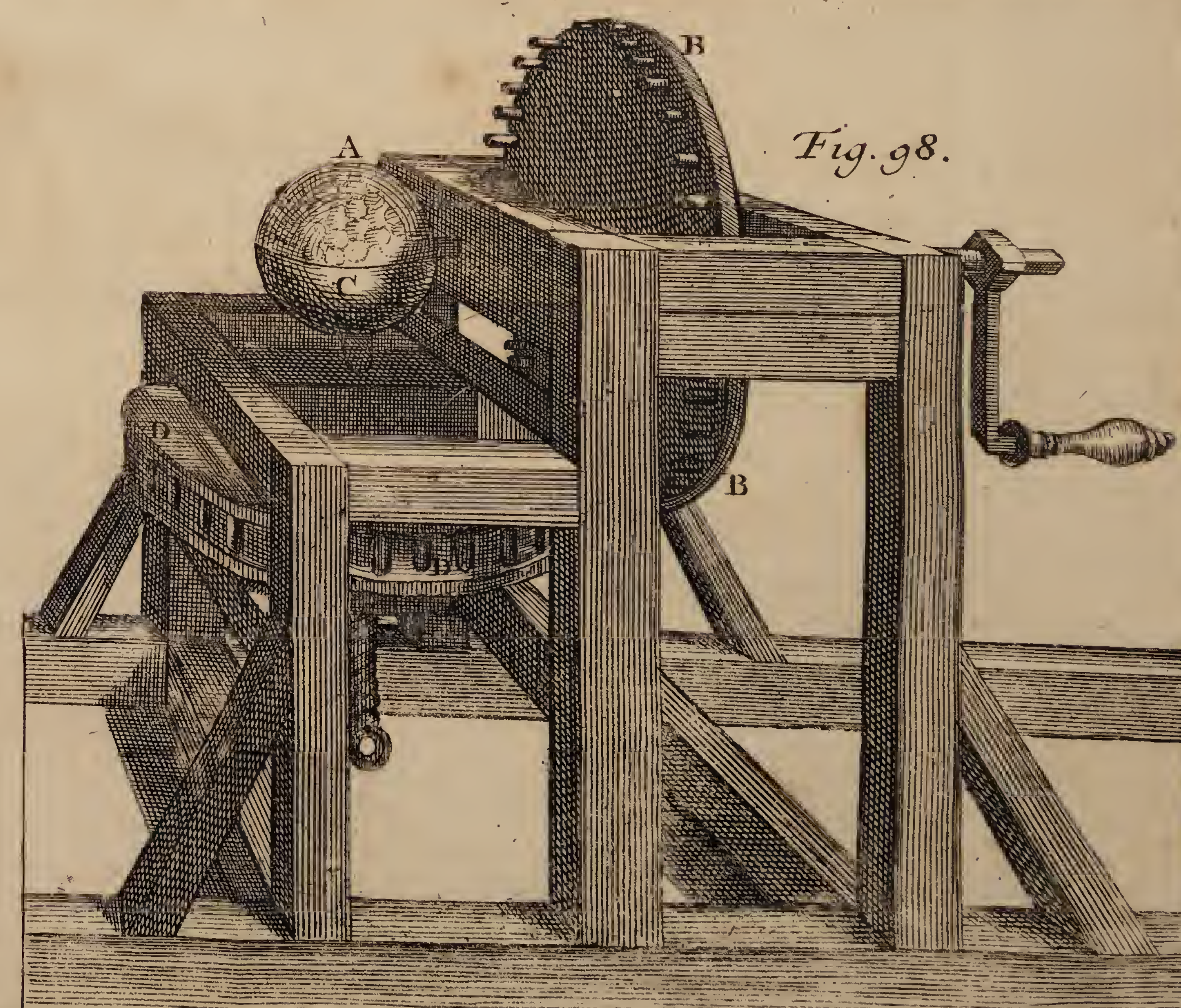


Fig. 98.



tho' moſt have rejected it, and ſome, concealing the Author's Name, have pretended it to be their own.

We ſhall therefore make our Attempt under the Conduct of theſe two Leaders; and in the firſt place propoſe under the name of *Phænomena* ſuch things, as either appear from Experiments, or are confirmed by certain Reaſonings drawn from them; and in the laſt place, we ſhall attempt the Solution of thoſe *Phænomena*.

Phænomena of the flowing of Water out of a Hole in the Bottom of a Veſſel conſtantly full.

1. The Depth of the Water, and the Time of flowing out being given, the *Meaſure* of the effluent Water is nearly in Proportion to the Hole.

2. The Depth of the Water, and alſo the Hole being given, the *Meaſure* of the effluent Water is in Proportion to the Time.

3. The Time of flowing out, and the Hole being given, the *Meaſure* of the effluent Water is nearly in a ſubduplicate Proportion to the Height of the Water.

4. The *Meaſure* of the effluent Water is nearly in a *Ratio* compounded of the Proportion of the Hole, the Proportion of the Time, and a ſubduplicate Proportion of the Depth of the Water.

5. The *Meaſure* of Water flowing out in a given Time is much leſs than that, which is commonly aſſigned by Mathematical Theorems. For the Velocity of effluent Water is commonly ſuppoſed to be that, which a heavy Body would acquire *in Vacuo* in falling from the whole Height of the Water above the Hole; and this being ſuppoſed, if we call the *Area* of the Hole F, the Height of the Water above the Hole A, the Velocity which a heavy Body acquires in falling *in Vacuo* from that Height V, and the Time of falling T, and if the Water flows out with this conſtant Velocity V, in the Time T; then the Length of the Column of Water, which flows out in that Time will be 2 A; and the *Meaſure* of it will be 2 A F. But if we calculate from the moſt accurate Experiments of *Poleni* *, we ſhall find the quantity of Water, which flows out in that Time, to be no more than about $\frac{571}{1000}$ of this *Meaſure* 2 A F.

This Illuſtrious Perſon's Experiments, are in my Opinion preferable to all others, not only becauſe of his extraordinary Diligence and Accuracy, but on other Accounts alſo. He found, that the Quantity of Water flowing out of a Veſſel thro' a cylindrical Tube far exceeded that, which flowed through a circular Hole made in a thin *Lamina*, the Tube and Hole being of equal Diameter, and the Height of the Water over both being alſo equal. And he found it to be ſo, when the Tube was inſerted, not into the Bottom, which others had obſerved before, but into the Side of the Veſſel.

Now a Hole made in the thinneſt *Lamina* muſt be conſidered as a ſhort cylindrical Tube. Whence it appears that a greater quantity of Water runs thro' a Hole made in a thin *Lamina*, than would have run

* *Polenus de Caſtellis*, Art. 35, 38, 39, 42, 43.

out, if the Thickneſs of the *Lamina* had been infinitely ſmall, as they expreſs themſelves. But as ſuch a *Lamina* can neither exiſt, nor even be conceived by the Imagination, it remains that we increaſe the Diameter of the Hole, that the Thickneſs of the *Lamina* may bear the leaſt Proportion poſſible to the Diameter of the Hole.

This *Poleni* performed with great Judgment, when he made uſe of a Diameter of 26 Lines, and a *Lamina* not quite a Line thick; whereas before him hardly any one made uſe of a Diameter of above 6 or 7 Lines, or ever attended to the Thickneſs of the *Lamina* or Bottom of the Veſſel, except Sir *I. Newton*, who mentions his making uſe of a very thin *Lamina*.

But *Poleni* exceeded all others, in conſidering not only the Size of the Hole but of the Veſſel alſo, that the Water might deſcend toward the Hole with the greateſt Freedom and leaſt Impediment, ſo that there can be no doubt but that the *Meaſures* taken by him come much nearer the Truth than any other.

6. Since, as we have juſt now ſeen the *Meaſure* of the Water running out in the above-mentioned Time T, is $2\,A\,F \times \frac{571}{1000}$, the Length of the Column of Water, which runs out in that Time, is $2\,A \times \frac{571}{1000}$. Therefore if each of the Particles of Water, which are in the Hole in the ſame Space of Time, paſſes with equal Velocity, it is plain that the common Velocity of them all is that with which the Space $2\,A \times \frac{571}{1000}$ would be gone over in the Time T, or the Velocity $V \times \frac{571}{1000}$. But this is the Velocity with which Water could ſpring *in Vacuo* to near $\frac{1}{3}$ of the Height of the Water above the Hole.

7. But when the Motion of Water is turned upwards, as in Fountains, the Fountains are ſeen to riſe almoſt to the entire Height of the Water in the Ciſtern. Therefore the Water, or at leaſt ſome Portion of the Water, ſpouts from the Hole with almoſt the whole Velocity V, and certainly with a much greater Velocity than $V \times \frac{571}{1000}$.

8. Hence it is evident, that the Particles of Water, which are in the Hole at the ſame Point of Time, do not all burſt out with the ſame Velocity, or have no common Velocity. The Mathematicians have hitherto taken the contrary to be certain.

9. At a ſmall Diſtance from the Hole, the Diameter of the Vein of Water is much leſs than that of the Hole. For Inſtance, if the Diameter of the Hole is 1, the Diameter of the Vein of Water will be $\frac{21}{25}$ or 0,84 according to Sir *I. Newton*'s Meaſure, who firſt obſerved this wonderful

wonderful *Phænomenon*; according to *Poleni*'s Meaſure $\frac{20}{26}$ or $\frac{20,5}{26}$; that is, if you take the mean Diameter, 0,78 nearly.

We ſhould now proceed to the Solution of theſe *Phænomena*; but before we do this, it will be convenient to acquaint the Reader with the following Particulars.

1. We conſider Water no otherwiſe than as a fluid, continuous, Body, the Parts of which yield to the leaſt Force, and are thereby moved amongſt themſelves.

2. By effluent Water we underſtand that Quantity of Water, which actually paſſes out of the Hole: and tho' it may ſeem unneceſſary, yet I have thought it proper to mention, that in my *Diſſertation on the Motion of running Waters*, inſerted about 24 Years ago in the *Philoſophical Tranſactions*, by defluent Water I underſtood that whole Quantity of Water, which is put in Motion within the Veſſel, and deſcends towards the Hole.

3. We conſider the Amplitude of the Veſſel as infinite, or at leaſt ſo great, that the Decreaſe of the Depth of Water therein in the whole Space of Time, in which the Water flows out of the Hole, is imperceptable.

4. We conſider Water as running out with a conſtant Velocity. At the beginning indeed of the Motion it runs out for a very ſmall Space of Time with a leſs Velocity than afterwards. But we paſs over the very beginning of the Motion, and inveſtigate the *Meaſure* and *Motion* of Water, when it has acquired it's utmoſt Velocity. Now this muſt neceſſarily be conſtant, as long as the Height of the ſuperincumbent Water remains the ſame.

5. We conceive the Bottom of the Veſſel no otherwiſe than as a Mathematical Plane, or at leaſt as ſo thin a *Lamina*, that it's Thickneſs is hardly any with regard to the Diameter of the Hole.

6. By the *Meaſure of effluent Water* in the following Pages we always underſtand that Quantity of Water, which flows out of the Hole in the ſame Space of Time that a heavy Body falling *in Vacuo* would take in paſſing through the Height of the Water above the Hole.

7. By the *Motion of effluent Water* we underſtand the Sum of the Motions of all the Particles of Water, which run out of the Hole in the above-mentioned Space of Time. But the Motion of every Particle is as the *Factum* of the Particle itſelf, and of the Velocity with which it burſts out of the Hole.

8. That what we ſhall ſay hereafter may be the more eaſily conceived, we ſhall firſt propoſe the more ſimple Caſes, and then proceed to thoſe which are more compound, but nearer to the true ſtate of things.

Thus in the firſt Problem, that the Solution may be the more ſimple, we ſuppoſe the Water to run out of the Hole into a *Vacuum*, and the

Particles

Particles of Water, whilſt they deſcend towards the Hole, to be without any Reſiſtance ariſing from a Defect of Lubricity.

In the ſecond and third Problem the Efflux of the Water is ſtill ſuppoſed to be *in Vacuo*, but we conceive the Particles of Water, whilſt they deſcend towards the Hole, to meet with ſome Reſiſtance for want of Lubricity, but ſo ſmall, that the Decreaſe of the *Motion* of the Water running out of the Hole occaſioned thereby, is to be accounted as nothing.

In the fourth and fifth we ſtill retain the the Suppoſition of the *Vacuum*; but the decreaſe of the *Motion* of the effluent Water for want of Lubricity is ſuppoſed to be ſenſible.

Laſtly, in the ſixth and following Problems, we conſider the thing as it really is, when it is tranſacted in the Air, ſo that the Particles of Water ſuffer a ſenſible Reſiſtance, not only from each other for want of Lubricity, within the Veſſel, but alſo after their going out of the Veſſel, from the Attrition of the ambient Air.

Prob. I. *To determine the* Motion, Meaſure, *and Velocity of Water running into a* Vacuum *thro' a Hole in the Bottom of a Veſſel, where the Particles of Water meet with no Reſiſtance for want of Lubricity.*

So long as the Hole is ſtopped, the Stopper ſuſtains the Weight of a Column of Water lying perpendicularly over it. On removing the Stopper, the Column of Water, which lies perpendicularly over it, being no longer ſuſtained, by it's Preſſure cauſes the Water to run out thro' the Hole, and after having brought it to it's due Velocity, keeps the Velocity of the effluent Water conſtant by it's conſtant Preſſure.

It muſt be conceived indeed, that the *Motion* of the Water running out of the Hole is derived not only from the Weight of the perpendicular Column, but partly from the Preſſure of this Column, and partly from the Preſſure of the ſurrounding Water. But this makes the *Motion* of the effluent Water neither greater nor leſs, than if it aroſe from the Preſſure only of the perpendicular Column: not leſs, becauſe the Preſſure of the perpendicular Column, if it is not obſtructed, will generate a *Motion* proportionable to itſelf, and it cannot be hindered but ſo far as the ſurrounding Water urges the effluent Water: not greater, becauſe the Preſſure of the ſurrounding Water can add nothing to the *Motion* of the effluent Water, unleſs it takes away as much from the Preſſure of the perpendicular Column.

Therefore the adequate Motion of the Water flowing out of the Hole is the Preſſure, or Weight, of the Column of Water over the Hole. But a given Force, howſoever applied, generates a given Quantity of Motion in a given Time, towards thoſe Parts whither the Force tends. Therefore the Weight of the incumbent Column generates a like Quantity of Motion in a given Time in the effluent Water, as it could generate in the ſame Time in the Column itſelf falling freely thro' a *Vacuum*.

Now becauſe, by the *Hypotheſis*, the Particles of Water find no Reſiſtance for want of Lubricity, and all thoſe Particles, which are juſt going out in the very Hole, are urged by an equal Preſſure of the ſuperincumbent Water, it is plain that the Velocity of all theſe is equal.

Let v be that common Velocity; a the Height, in falling from which *in Vacuo* that Velocity would be acquired; A the Height of the Water above the Hole; V the Velocity acquired by falling *in Vacuo* from the Height A; T the Time of falling from the ſame Height; F the *Area* of the Hole; and let the Water flow out of the Hole in the Time T.

Now becauſe in the Time T, with the Velocity V, the Space 2 A will be run over, the Space $\frac{2\,A\,v}{V}$ will be run over in the ſame Time, with the Velocity v. Therefore this will be the Length of the Column of Water, which flows out of the Hole in the Time T; and the Magnitude of this Column, or the *Meaſure* of the Water flowing out in the Time T, will be $\frac{2\,A\,v\,F}{V}$ and the *Motion* of the ſame will be $\frac{2\,A\,F\,v^2}{V}$.

But the *Motion*, which can be generated in the Column of Water over the Hole, in the ſame Time T, if carried by it's own Weight thro' a *Vacuum* is thus.

It's Velocity will be V, and as it's Magnitude is A F, it's *Motion* will be A F V.

But that *Motion*, from what has been ſaid above, is equal to the *Motion* of the Column of Water flowing out in the Time T, or $A\,F\,V = \frac{2\,A\,F\,v^2}{V}$.

Hence $V = \frac{2\,v^2}{V}$, or $v^2 = \frac{V^2}{2}$, and $v = \frac{V}{\sqrt{2}}$.

Moreover the Meaſure aſſigned above of the Water running out in the Time T, or $\frac{2\,A\,F\,v}{V} = \frac{2\,A\,F}{V} \times \frac{V}{\sqrt{2}} = \frac{2\,A\,F}{\sqrt{2}} = A\,F \times \sqrt{2}$. Q. E. I.

Since $a : A :: v^2 : V^2$; therefore $a = \frac{A\,v^2}{V^2}$, that is, $a = \frac{A}{V^2} \times \frac{V^2}{2}$, or $a = \frac{A}{2}$. Therefore the Height a, which the effluent Water can reach by turning the Motion upwards, is half the Height of the Water in the Veſſel above the Hole; which is the very Height determined by Sir *I. Newton*, *Princip. Ed.* 3. *Lib.* 2. *Prop.* 36. Coroll. 1.

If

Coroll. 2. If we afcribe to the effluent Water that Velocity, which is acquired by falling from the whole Height of the Water above the Hole, that is, if we fuppofe the Velocity $v = V$, then the above determined Motion of the Water $\frac{2\,A\,F\,v^2}{V} = 2\,A\,F\,V$, or double that Motion, which can be generated by the Column over the Hole, and therefore not to be generated but by double this Column; as we are taught by Sir *I. Newton*, *Princip. Ed.* 2 and 3. *Lib.* 2. *Prop.* 36.

Scholium. This *Meafure* here determined $\frac{2\,A\,F}{\sqrt{2}}$, or $2\,A\,F \times 0{,}707$, as it falls far fhort of that which is generally determined by Mathematicians, namely $2\,A\,F$, fo it far exceeds that *Meafure* which is fhewn by *Poleni*'s Experiments, or $2\,A\,F \times 0{,}571$, and no wonder, for what is fuppofed in this Problem, that the Particles of Water find no Refiftance in running down, the *Hypothefis* is far from the true ftate of things.

Prob. II. *To determine the* Motion, Meafure, *and Velocity of Water running out into a* Vacuum *thro' a circular Hole in the middle Part of the Bottom of a cydindrical Veffel, where the Particles of Water find fome Refiftance for want of Lubricity, but fo fmall, that the Decreafe of the* Motion *of the effluent Water occafioned thereby cannot be accounted any thing.*

Fig. 99. Let A B C D be an immenfe cylindrical Veffel, E F a circular Hole made in the middle Part of the Bottom, and the Water being perfectly at Reft and unmoved in the Veffel, let the Stopper be removed from the Hole, that a Paffage may be opened for the Water thro' the Hole.

Then becaufe the Water has been hitherto unmoved, and now begins to run out thro' the Hole, and the Water placed above follows that which runs out, and the natural Motion of the Water is not difturbed by pouring any over it, and the Hole is in the very middle of the Bottom, that Portion of Water, which is in Motion, and defcends towards the Hole, will neceffarily affume fome regular Figure A H E F K B, of which the lower Bafe is the Hole itfelf, and the upper Bafe, the upper Surface of the Water A B, and all the horizontal Sections are circular. We call this a *Cataract*, but we do not yet examine what is the Figure of the *Cataract*: it is fufficient for our prefent Defign, to obferve that it is regular, and that the fame Quantity of Water paffes in a given Time thro' each of it's horizontal Sections.

Now becaufe all that Water which tends downwards, is contained in the *Cataract*, it follows that the reft of the Water A H E C, B K F D, which is without the *Cataract*, has no Motion at all, and is perfectly at Reft. Therefore in any horizontal Section of the *Cataract* H *c* K, whofe Centre is *c* the Points H, K fhall reprefent the Bounds between the Water defcending towards the Hole, and the furrounding quiefcent Water.

Moreover

Moreover, as the Point K is the Bound of Motion and Reſt, and the Particles of Water, whilſt they are in Motion, find a Reſiſtance for want of Lubricity, the Particle of Water α within the *Cataract*, and next to the Point K, muſt be carried downwards only with the leaſt Velocity. Otherwiſe it would neceſſarily carry with it the next Particle a, placed without the *Cataract*, contrary to the *Hypotheſis*. But the Particle β, which is contiguous within to the Particle α, will not deſcend but with the leaſt relative Velocity with regard to the Particle α; becauſe otherwiſe it would carry the Particle α away with it by accelerating it, and this Particle α, being now in a quicker Motion, would carry away with it the Particle a. In like manner the Particle γ being placed more within, and contiguous to the Particle β, will deſcend with the leaſt relative Velocity with regard to the Particle β, and the other Particles δ, ε, &c. being placed one more within than another, will deſcend with the leaſt relative Velocity with regard to each of the Particles lying next to each of them without. And by this means the abſolute Velocity of the Particles muſt neceſſarily increaſe gradually from the bound toward the Centre c, that the Velocity of the Water may be greateſt in the very Centre, and leaſt at each Bound K and H. Fig. 100.

But it is neceſſary that the Reſiſtance, which each quicker Particle finds from the Friction of the adjacent ſlower Particle placed without, ſhould be perpetually equal thro' the whole Section of the *Cataract*. Otherwiſe that Particle, which finds the greater Reſiſtance, will accelerate the adjacent ſlower Particle, till the Reſiſtance is by this means diminiſhed, and becomes equal to that Reſiſtance, which is found by the other Particles. But if the Reſiſtance is equal every where thro' the whole Section of the *Cataract*, the relative Velocity of the Particles will be alſo equal every where, when one of them neceſſarily follows another.

Therefore the abſolute Velocity of every Particle, which is the Sum of all the relative Velocities, from the Circumference of the Section to that very Particle, taken all together, is in the *Ratio* of the Diſtance of the ſame Particle from the Circumference of the *Cataract*.

Now let r be the *Radius* of the Hole, m to 1 in the Proportion of the Circumference to the Diameter, $m r^2$ the Area of the Hole, v the Velocity with which the Water deſcends in the Centre of the Hole, a the Height by falling from which *in Vacuo* the Velocity v is acquired, A the Height of the Water above the Hole, V the Velocity acquired by falling *in Vacuo* from the Height A, T the Time of falling from the ſame, z the Diſtance of every Particle from the Centre of the Hole, and let the Water run out in the Time T.

Now the *Meaſure* of the Water, which goes out of the Hole in the Time T, will be found after this manner: z will be the *Radius* of every Circle within the Hole, $2mz$ the Circumference of the ſame, $2mzz$ the

the *annulus nascens* adjacent to that Circumference, $\frac{v \times \overline{r-z}}{r}$ the Velocity of the Water in the *annulus nascens*.

Since V : $v \times \frac{\overline{r-z}}{r}$: : 2 A : $\frac{2\,A\,v \times \overline{r-z}}{V\,r}$, therefore $\frac{2\,A\,v \times \overline{r-z}}{V\,r}$ will be the Space, which the Water makes in flowing thro' the *annulus nascens* in the Time T, and the *Measure* of the same Water will be $2\,m\,z\,\dot{z} \times \frac{2\,A\,v \times \overline{r-z}}{V\,r} = \frac{4\,m\,A\,v \times \overline{r\,z\,\dot{z} - z^2\,\dot{z}}}{V\,r}$.

But the *Measure* of the Water passing thro' the *annulus nascens* is the Fluxion of the *Measure* of the Water passing thro' a Circle whose *Radius* is z. Therefore the *Measure* of the Water, which passes thro' this Circle in the Time T, is the fluent quantity of the Fluxion just now mentioned $\frac{4\,m\,A\,v}{V\,r} \times \overline{r\,z\,\dot{z} - z^2\,\dot{z}}$, that is $\frac{4\,m\,A\,v}{V\,r} \times \frac{\overline{3\,r\,z^2 - 2\,z^3}}{6}$ $= \frac{2\,m\,A\,v}{3\,V\,r} \times \overline{3\,r\,z^2 - 2\,z^3}$. And supposing $z = r$, the *Measure* of the Water passing thro' all the Hole in the Time T will be found, namely $\frac{2\,m\,A\,v\,r^2}{3\,V}$.

But the Motion of the same Water will be found thus.

The *Measure* of the Water running thro' the *annulus nascens* in the Time T is, as we have just now seen, $\frac{4\,m\,A\,v}{V\,r} \times \overline{r\,z\,\dot{z} - z^2\,\dot{z}}$, and as it's Velocity $v \times \frac{r-z}{r}$, it's *Motion* will be $\frac{4\,m\,A\,v}{V\,r} \times \overline{r\,z\,\dot{z} - 2\,z^2\,\dot{z}}$ $\times \frac{v}{r} \times \overline{r-z} = \frac{4\,m\,A\,v^2}{V\,r^2} \times \overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} \times z^3\,\dot{z}}$, the fluent Quantity of which is $\frac{4\,m\,A\,v^2}{V\,r^2} \times \overline{\frac{r^2\,z^2}{2} - \frac{2\,r\,z^3}{3} + \frac{z^4}{4}} = \frac{m\,A\,v^2}{3\,V\,r^2}$ $\times \overline{6\,r^2\,z^2 - 8\,r\,z^3 + 3\,z^4}$, which is the *Motion* of the Water flowing thro' a Circle whose *Radius* is z. And supposing $z = r$, we have the *Motion* of the Water running out in the Time T thro' all the Hole, $\frac{m\,A\,v^2\,r^2}{3\,V}$.

But this *Motion*, by the Solution of Prob. I, and by the *Hypothesis* of this, is equal to the *Motion*, which the Column over the Hole can

acquire in the fame Time T, by falling with it's own Weight thro' a *Vacuum*, that is to the *Motion* A F V, or A V $\times m r^2$. Therefore

$$\frac{m A v^2 r^2}{3 V} = m A V r^2.$$

Hence $v^2 = 3 V^2$ and $v = V \times \sqrt{3}$.

Moreover the above-mentioned Meafure of the Water running out thro' the Hole in the Time T, namely $\frac{2 m A v r^2}{3 V} = \frac{2 m A r^2}{3 V} \times V \times \sqrt{3} = \frac{2 A m r^2}{\sqrt{3}}$ Q. E. I.

Since $V^2 : v^2 :: A : a$, therefore $a = \frac{A v^2}{V^2} = \frac{A}{V^2} \times 3 V^2 = 3 A$. *Coroll.* 1.

Therefore the Height, to which the Water can rife with that Velocity, with which it runs out in the Centre of the Hole, is triple the Height of the Water above the Hole.

The Figure of the *Cataract* will be determined after the following manner. *Coroll.* 2.

Let H K be any Section of a *Cataract*, whofe Centre is c, and let it's *Radius* be c K $= y$, the Height of the Water above that Section, or I $c = x$, t the Time of falling *in Vacuo* from the Height x, and, as before, let L F $= r$, and I L $=$ A. Fig. 101.

Now the Water paffes thro' this Section H K in the fame Quantity as it runs out of the Hole E F.

But if the Veffel is fhortened, fo that it's Height is reduced from I L to I c, and fo that Section H K now becomes the very Hole in the Bottom of the Veffel, the Water will pafs in a given Time, thro' this Section, in a Quantity neither greater nor lefs than it paffed before thro' the fame, before the Veffel was fhortened: not greater, becaufe that Section is preffed only by the fame Weight of the fuperincumbent Column, by which it was preffed before; not lefs, becaufe the lower Water H K F E does not hinder the Motion of the Water, as it paffes thro' the Section H K.

Now the Veffel being fhortened, the *Meafure* of the Water running out of the Hole H K in the Time t, by the preceding Solution, is $\frac{2 x m y^2}{\sqrt{3}}$, and the *Meafure* of the Water running out in the Time T is $\frac{2 x m y^2}{\sqrt{3}} \times \frac{T}{t} = \frac{2 x m y^2}{\sqrt{3}} \times \frac{\sqrt{A}}{\sqrt{x}}$. For $T : t :: \sqrt{A} : \sqrt{x}$.

But from what has been faid above, the *Meafure* of the Water running out of the Hole H K, when the Veffel is fhortened in the given Time T, is equal to the *Meafure* of the Water paffing in the fame Time

 thro'

thro' the Section H K, when the Veſſel is entire, or to the *Meaſure* of the Water running out of the Hole E F in the ſame Time. Therefore $\frac{2\,x\,m\,y^2}{\sqrt{3}} \times \frac{\sqrt{A}}{\sqrt{x}} = \frac{2\,A\,m\,r^2}{\sqrt{3}}$, or $y^2\sqrt{x} = r^2\sqrt{A}$, or $y^4 x = r^4 A$, which is the very Equation of the hyperpolical Curve, by the Rotation of which I formerly ſhewed the Figure of the *Cataract* to be generated *.

Schol. I. The *Meaſure* of the Water now found $\frac{2\,A\,m\,r^2}{\sqrt{3}}$, or $2\,A\,m\,r^2 \times 0{,}577350$ is a ſmall matter greater than the *Meaſure* $2\,A\,m\,r^2 \times 0{,}571$, which is obtained from *Polcni*'s Experiments. But this difference, at leaſt in ſome Part, proceeds hence, that in this Problem the Decreaſe of the *Motion* of the Water ariſing from Reſiſtance is accounted for nothing.

Schol. II. The *Meaſure* of the effluent Water determined by this Solution is right, if we conſider the Height of the Veſſel as infinitely great with Regard to the Diameter of the Hole. But as this Height has a finite Proportion to the Diameter of the Hole, the *Meaſure* will be ſomething leſs, ſo that, when the Height is 5 times greater than the Diameter, it will differ from the truth only $\frac{1}{32000}$, and when it is double, only about $\frac{1}{5120}$, which Differences are ſmaller than can be diſcovered by any Experiment.

But this very ſmall Difference proceeds from this, that the abovementioned relative Velocity, and therefore the abſolute Velocity of the Particles of Water, which we have conſidered as in a Direction perpendicular to the Horizon, are really in a Direction ſomething oblique, when every Particle comes nearer to the *Axis* of the *Cataract* in deſcending.

But if any one deſires to obtain a true and accurate Solution, when the Altitude of the Water has any Proportion whatſoever to the Diameter of the Hole, it may be done after the following manner.

From the property of the *cataractic* Curve explained in Cor. 2. of this Problem, by which $y^4 x = r^4 A$, the Subtangent of this Curve will be found to be to the Circumference of the Hole 4 A, and to the Circumference of any Section the Subtangent will be $4\,x$, that is, equal to the Height of the Water above that Section taken 4 times.

But ſuch a *cataractic* Curve is diſcribed not only by the outer Water, which flows beyond the Circumference of the Hole, but alſo by that Part of the Water, which flows thro' any *Annulus* of the Hole; that is, every Particle of Water deſcribes ſuch a Curve.

Now let z be the Diſtance of any Particle placed in the Hole from the Centre of the Hole, and let this Particle deſcend thro' the leaſt

* Vol. IV. Part. 1. Ch. V. §. iv.

Space

Space imaginable in a Tangent to the *cataractic* Curve. Hence it's Velocity will be in the Direction of this Tangent, or the Velocity $v \times \frac{\overline{r-z}}{r}$ explained in this Problem to the Velocity of the same in a Direction Perpendicular to the Horizon as $\sqrt{16\,A^2 + z^2} : 4\,A$.

Therefore the Velocity in a Direction perpendicular to the Horizon is $v \times \overline{r-z} \times \frac{4\,A}{\sqrt{16\,A^2+z^2}}$.

Hence also, by following the Steps of the above Solution, you will have for the *Measure* of the Water passing thro' the *annulus nascens* $\frac{16\,m\,A^2\,v}{r\,V} \times \frac{r\,z\,\dot{z} - z^2\,\dot{z}}{\sqrt{16\,A^2+z^2}}$.

Now the fluent Quantity of this Fluxion will be found, by the Cotesian Measures of *Ratios Form.* V. and VI. to be $\frac{16\,m\,A^2\,v}{r\,V}$

$$\times \overline{\frac{2\,r-z}{2}\sqrt{16\,A^2+z^2}+8\,A^2} \left| \frac{z+\sqrt{16\,A^2+z^2}}{4\,A} \right.$$ and

by making first $z = o$, and then $z = r$, you will have $\frac{16\,m\,A^2\,v}{r\,V}$

$$\times \overline{\frac{r}{2}\sqrt{16\,A^2+r^2} - 4\,A\,r + 8\,A^2} \left| \frac{r+\sqrt{16\,A^2+r^2}}{4\,A} \right.$$ for the Measure of the Water passing thro' all the Hole in the Time T.

Moreover, by proceeding after the same manner, you will have for the Motion of the Water passing thro' the *annulus nascens* $\frac{64\,m\,A^3 v^2}{r^2\,V}$

$$\times \frac{\overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}}}{16\,A^2+z^2}.$$ Of which Fluxion the fluent Quantity,

by the Cotesian *Form.* I and II, will be found $\frac{64\,m\,A^3\,v^2}{r^2\,V}$ in $\frac{z^2 - 4\,r\,z}{2}$

$$+ \frac{r^2}{2} \left| \frac{16\,A^2+z^2}{16\,A^2} - \frac{16\,A^2}{2} \right| \frac{16\,A^2+z^2}{16\,A^2} + 2\,r\sqrt{-16\,A^2} \left| \frac{z+\sqrt{-16\,A^2}}{\sqrt{16\,A^2+z^2}} \right.,$$ and by supposing $z = r$, you will have $\frac{64\,m\,A^3\,v^2}{r^2\,V}$

in

in $\frac{r^2 - 16\,A^2}{2} \Big| \frac{16\,A^2 + r^2}{16\,A^2} + 2\,r\sqrt{-16\,A^2} \Big| \frac{r + \sqrt{-16\,A^2}}{\sqrt{16\,A^2 + r^2}}$

$-\frac{3\,r^2}{2}$, which is the *Motion* of the Water paſſing thro' the Hole in the Time T.

Now let $M = \frac{r}{2}\sqrt{16\,A^2 + r^2}$,

$$N = 8\,A^2 \Big| \frac{r + \sqrt{16\,A^2 + r^2}}{4\,A}, \text{ or}$$

$$N = 4\,A^2 \Big| \frac{16\,A^2 + 2\,r^2 + 2\,r\sqrt{16\,A^2 + r^2}}{16\,A^2}$$

$$K = \frac{r^2 - 16\,A^2}{2} \Big| \frac{16\,A^2 + r^2}{16\,A^2}, \text{ and}$$

$$L = 2\,r\sqrt{-16\,A^2} \Big| \frac{r + \sqrt{-16\,A^2}}{\sqrt{16\,A^2 + r^2}}, \text{ or}$$

$L = 2\,r \times 4\,A$ (Rad : Tang : Sec : : $4\,A : r : \sqrt{16\,A^2 + r^2}$) and the *Meaſure* of the Water paſſing thro' the Hole in the Time T will be $\frac{16\,m\,A^2\,v}{r\,V} \times \overline{M + N - 4\,A\,r}$; but the Motion of the ſame Water will be $\frac{64\,m\,A^3\,v^2}{r^2\,V} \times \overline{L + K - \frac{3\,r^2}{2}}$.

But $\frac{64\,m\,A^3\,v^2}{r^2\,V} \times \overline{L + K - \frac{3\,r^2}{2}} = m\,r^2\,A\,V$, wherefore $v^2 = \frac{r^4\,V^2}{64\,A^2 \times \overline{L + K - \frac{3\,r^2}{2}}}$; and the Meaſure of the Water running thro' the Hole in the Time T is $2\,m\,A\,r \times \frac{M + N - 4\,A\,r}{\sqrt{L + K - \frac{3r^2}{2}}}$.

But if inſtead of the *Meaſures* of *Ratio's* and Angles, you would rather make uſe of infinite *Series*, the above *Meaſure* of the Water running thro' the *annulus naſcens*, $\frac{16\,m\,A^2\,v}{r\,V} \times \frac{r\,z\,\dot{z} - z^2\,\dot{z}}{\sqrt{16\,A^2 + z^2}}$, muſt be reduced to this Form, $\frac{m\,v}{r\,V} \times \overline{r\,z\,\dot{z} - z^2\,\dot{z}} \times \frac{16\,A^2}{\sqrt{16\,A^2 + z^2}}$; and by reducing

$\frac{16\,A^2}{\sqrt{16\,A^2 + z^2}}$ to an infinite Series, you will have $\frac{m\,v}{r\,V} \times \overline{r\,z\,\dot{z} - z^2\,\dot{z}}$ in $4\,A - \frac{z^2}{8A} + \frac{3\,z^4}{8^3\,A^3} - \frac{5\,z^6}{4 \times 8^4\,A^5} + \frac{35\,z^8}{8\;A^7}$, — &c. for the *Measure* of the Water running thro' the *annulus nascens*; and by the fluent Quantity of this Fluxion, or by $\frac{m\,v}{V}$ in $\frac{2\,A\,r^2}{3} - \frac{r^4}{20 \times 8\,A} + \frac{r^6}{14 \times 8^3\,A^3} - \frac{5\,r^8}{36 \times 8^5\,A^5} + \frac{7\,r^{10}}{22 \times 8^7\,A}$, &c. we shall have the *Measure* of the Water running out thro' all the Hole.

Moreover the above *Motion* of the Water passing thro' the *annulus nascens* $\frac{64\,m\,A^3\,v^2}{r^2\,V} \times \frac{\overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}}}{16\,A^2 + z^2} = \frac{4\,m\,A\,v^2}{r^2\,V}$

$\times \overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}} \times \frac{16\,A^2}{16\,A^2 + z^2} = \frac{4\,m\,A\,v^2}{r^2\,V}$

$\times \overline{r^2\,z\,\dot{z} - 2\,r\,z^2\,\dot{z} + z^3\,\dot{z}}$ in $1 - \frac{z^2}{16\,A^2} + \frac{z^4}{16^2\,A^4} - \frac{z^6}{16^3\,A^6} + \frac{z^8}{16^4\,A^8} - \frac{z^{10}}{16^5\,A^{10}}$, + &c. and by the fluent Quantity of this Fluxion, or by $\frac{4\,m\,A\,v^2}{V}$ in $\frac{r^2}{12} - \frac{r^4}{60 \times 16\,A^2} + \frac{r^6}{168 \times 16^2\,A^4} - \frac{r^8}{360 \times 16^3\,A^6} + \frac{r^{10}}{660 \times 16^4\,A^8}$ — &c. we shall have the *Motion* of the Water running out thro' all the Hole.

Therefore $A\,m\,r^2\,V = \frac{4\,m\,A\,v^2}{V}$ in $\frac{r^2}{12} - \frac{r^4}{60 \times 16\,A^2} +$ &c. or

$$V^2 = v^2 \text{ in } \frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \text{\&c. or}$$

$$v^2 = \frac{V^2}{\frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \text{\&c.}}$$

$$\text{and } v = \sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \text{\&c.}}$$

Whence the *Measure* of the Water running out thro' the Hole, or m

$$\frac{m\,v}{V} \text{ in } \frac{2\,A\,r^2}{3} - \frac{r^4}{20 \times 8\,A} + \frac{r^6}{14 \times 8^3\,A^3} - \frac{5\,r^8}{36 \times 8^5\,A^5} + \&c.$$

$$= \frac{m}{V} \text{ in } \frac{2\,A\,r^2}{3} - \frac{r^4}{20 \times 8\,A} + \frac{r^6}{14 \times 8^3\,A^3} - \frac{5\,r^8}{36 \times 8^5\,A^5} + \&c.$$

$$\times \frac{V}{\sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \&c.}}$$

$$= m \text{ in } \frac{\frac{2\,A\,r^2}{3} - \frac{r^4}{20 \times 8\,A} + \&c.}{\sqrt{\frac{1}{3} - \frac{r^2}{15 \times 16\,A^2} + \&c.}}$$

Whence at length the *Meaſure* of the Water running out of the Hole is found to be $\frac{2\,A\,m\,r^2}{\sqrt{3}}$ in $1 - \frac{r^2}{20 \times 16\,A^2} + \frac{r^4}{56 \times 16^2\,A^4} - \&c.$ Hence by ſuppoſing A infinite with reſpect to the Diameter of the Hole, the *Meaſure* comes out $= \frac{2\,A\,m\,r^2}{\sqrt{3}}$, as we have determined in this Problem.

When $A = 10\,r$, the *Meaſure* $= \frac{2\,A\,m\,r^2}{\sqrt{3}} \times 1 - \frac{1}{32000}$, or thereabouts.

When $A = 4\,r$ the *Meaſure* $= \frac{2\,A\,m\,r^2}{\sqrt{3}} \times 1 - \frac{1}{5120}$, or thereabouts.

Therefore inſtead of the true *Meaſure*, we may take the *Meaſure* $\frac{2\,A\,m\,r^2}{\sqrt{3}}$, without Danger of any ſenſible Error, even in ſo ſmall an Altitude, and much more in an Altitude many times greater, as it is uſually in Experiments; and by this means the Computation, from being very laborious and intricate, becomes moſt eaſy.

Prob. III. *The ſame being ſuppoſed, and neglecting the Acceleration of the Water without the Hole, to determine the Diameter of the Vein of Water to the ſmall Diſtance without the Hole, where the Vein is moſt contracted, and the Velocity of the Water in the Vein ſo contracted.*

In the Solution of the former Problem it was ſhewn, that the Particles of Water burſting out of the Hole, do not come forth with one Velocity common to them all, but with the greater Velocity as they are leſs Diſtant from the Centre of the Hole; and that the relative Velocity of the

the inner Particles, with regard to the Particles that touch each of them on the outside, is constantly equal thro' all the Hole; and that this relative Velocity proceeds from the Resistance, which the Water finds, as it descends toward the Hole, from the surrounding Water.

But after the Water is gone out of the Hole, and it's outer Surface no longer finds any Resistance from the surrounding Water, nor from the ambient Air, being carried thro' a *Vacuum* by the *Hypothesis*, that relative Velocity, or Inequality of absolute Velocity, can no longer remain. For now the swifter Particles must necessarily accelerate the slower contiguous Particles, and must also themselves be retarded by the slower, till they have all acquired one Velocity common to all the Particles: Which will happen within a small Space after their being come out of the Hole.

But whilst all the Particles are acquiring this common Velocity, the Diameter of the Vein must necessarily be contracted. This happens in the same manner, as when a rapid River is joined with a slower, for Instance the *Rhone* with the *Saone*. In the common Channel the Velocity of the Water brought from both Rivers is equal, and the Water is transmitted thro' a Section of this Channel in like Quantity as it was before transmitted thro' the Sections of both Rivers: But a Section of the *Rhone*, after it has received the *Saone*, is far less than the Sum of the Sections of the *Rhone* and of the *Saone*, before their Conflux.

Therefore let the *Radius* of the contracted Vein of Water, where all the Particles in the same Section of the Vein have acquired an equal Velocity, be ϱ, and let that common Velocity be called υ.

Now the *Measure* of the Water flowing thro' a Section of the contracted Vein in the Time T will be thus.

$V : \upsilon :: 2A : \frac{2A\upsilon}{V}$, which is the Length of the Vein of Water passing thro' this Section in the Time T. And $\frac{2A\upsilon}{V} \times m\varrho^2$ is the *Measure* of the Water passing thro' this Section in the same Time.

And the *Motion* of the Water passing thro' the Section of the Vein in the Time T is $\frac{2A\upsilon}{V} \times m\varrho^2 \times \upsilon$, or $\frac{2Am\varrho^2\upsilon^2}{V}$.

But the *Measure* of the Water passing thro' the Section of the Vein is equal to the *Measure* of the Water running out thro' the Hole in the same Time, that is, $\frac{2Am\varrho^2\upsilon}{V} = \frac{2Amr^2}{\sqrt{3}}$, or $2\varrho^2\upsilon = \frac{2r^2V}{\sqrt{3}}$.

Moreover the *Motion* of the Water bursting out of the Hole, as it is not altered by the Action of the Particles on each other, will be equal to the Motion of the Water running thro' the Section of the Vein,

 that

that is $A\,V\,m\,r^2 = \frac{2\,A\,m\,\varrho^2\,v^2}{V}$, or $2\,\varrho^2\,v^2 = r^2\,V^2$.

But $v = \frac{2\,\varrho^2\,v^2}{2\,\varrho^2\,v} = r^2\,V^2 \times \frac{\surd 3}{2\,r^2\,V^2}$, that is $v = \frac{V\surd 3}{2}$, and $v^2 = \frac{3\,V^2}{4}$.

And $\varrho^2 = \frac{r^2\,V^2}{2\,v^2} = \frac{r^2\,V^2}{2} \times \frac{4}{3\,V^2}$, or $\varrho^2 = \frac{2\,r^2}{3}$, and $\varrho = \frac{r\,\surd 2}{\surd 3}$

Q. E. I.

Coroll. Since $v^2 = \frac{3\,V^2}{4}$, and the Altitudes are in a duplicate *Ratio* of the Velocities generated by falling from thence, it is manifeſt, that this is the Velocity of the Water in the contracted Vein, by which it can riſe upwards *in Vacuo* to ¾ of the Height of the Water above the Hole.

Schol. I. This wonderful Contraction of the Vein of Water was firſt of all diſcovered, about 30 Years ago, by Sir *I. Newton*, when he was conſidering the Motion of effluent Water more attentively, on Account of ſome Difficulties propoſed by Mr *Cotes*, who was then taking care of the ſecond Edition of the *Principia*; and *Poleni*, afterwards confirmed it by many Experiments. From that Time this *Phænomenon* has more than enough exerciſed the Wits of Philoſophers: But the true Cauſe of this Contraction has hitherto eſcaped them all.

The *Radius* of the Vein determined by this Problem $\frac{r\surd 2}{\surd 3}$, or $r \times 0{,}8165$, is a little leſs than the *Radius* $r \times 0{,}84$, delivered by Sir *Iſaac*: and a little greater than the *Radius* $r \times 0{,}78$, according to *Poleni*'s Meaſure, and is almoſt a mean between them both.

But the Velocity above determined $\frac{V\surd 3}{2}$, by which the Water can riſe upwards to ¾ of the Height of the Veſſel above the Hole, differs very far from the Experiments, by which Fountains are found to riſe to almoſt the entire Height of the Ciſtern. Now that Difference of Velocity, proceeds from the Reſiſtance of the ambient Air, which is ſo far from diminiſhing the Height of the Spout, as is commonly believed, that it does not a little increaſe it, as will appear from the Solution of *Prob.* VII.

Schol. II. From what has been ſaid above in *Prob.* II. *Schol.* 2. it appears that theſe Values of ϱ and v, cannot be accounted accurate, unleſs the Altitude of the Water be accounted infinite with regard to the Diameter of the Hole, but that they approach very near to the true Values if

if the Altitude of the Water is double, or more than double the Diameter of the Hole. But if you would accurately determine the same Values, you may use the *Measure* determined in the same *Scholium* or $2\,m\,A\,r \times \frac{\overline{M + N - 4\,A\,r}}{\sqrt{L + K - \frac{3}{2}r^2}}$, whence you will have $v = \frac{r\,V}{2} \times \frac{\sqrt{L + K - \frac{3}{2}r^2}}{M + N - 4\,A\,r}$, and $\varrho = \sqrt{2} \times \frac{\overline{M + N - 4\,A\,r}}{\sqrt{L + K - \frac{3}{2}r^2}}$. You may also make use of the infinite Serieses in the same Scholium.

The Water running out from a circular Hole in the Middle of the Bottom of a cylindrical Vessel, where the Particles of Water in running down within the Vessel find so great a Resistance from the Want of Lubricity, that the Motion of the Water is thereby remarkably diminished, as also the given Measure *of the effluent Water, to determine the* Motion *of the same, and the Velocity with which it goes out through the Middle of the Hole.* Prob. IV.

Let the given *Measure* of the Water running out in the Time T be $2\,m\,r^2\,A\,q$. Therefore the *Measure* assigned by *Analysis* in the Solution of Prob. II. will be equal to it, namely $\frac{2\,m\,r^2\,A\,v}{3\,V}$, that is $2\,m\,r^2\,A\,q = \frac{2\,m\,r^2\,A\,v}{3\,V}$, or $v = 3\,V\,q$.

But the *Motion* of the same Water assigned by the *Analysis* in the same Problem is $\frac{m\,r^2\,A\,v^2}{3\,V}$, and by substituting instead of v^2 it's Value just now found, that *Motion* becomes $\frac{m\,r^2\,A}{3\,V} \times 9\,V^2\,q^2 = 3\,q^2\,m\,r^2\,A\,V$. Q. E. I.

If from the *Motion*, which can be generated in the Time T by the Column of Water over the Hole, or from $m\,r^2\,A\,V$, be subtracted, the *Motion* of the Water running out in the same Time, $3\,q^2\,m\,r^2\,A\,V$, there remains *the Motion* lost in the Time T by the Resistance $m\,r^2\,A\,V \times \overline{1 - 3\,q^2}$. Coroll.

If you desire an accurate Solution, you must have Recourse to *Prob.* II. *Schol.* 2. after this Manner; $2\,m\,r^2\,A\,q = \frac{16\,m\,A^2\,v}{r\,V} \times \overline{M + N - 4\,A\,r}$, whence $v = V\,q \times \frac{r^3}{8\,A \times \overline{M + N - 4\,A\,r}}$. And the *Motion* Schol.

 of

of the Water running out in the Time T will be $m\,r^2\ A\,V \times q^2\,r^2 \times \overline{\dfrac{L+K-\frac{3}{2}\,r^2}{M+N-4\,A\,r|^2}}$, whence the *Motion* loſt by the Reſiſtance in the Time T will be $m\,r^2\ A\,V \times \overline{1-\dfrac{q^2\,r^2 \times L+K-\frac{3}{2}\,r^2}{M+N-4\,A\,r|^2}}$.

Prob. V. *With the ſame Poſitions and* Data, *and neglecting the Acceleration of the Water without the Hole, to determine the Diameter of the Vein of Water at a ſmall Diſtance without the Hole, where the Vein is moſt contracted, and the Velocity of the Water in the Vein ſo contracted*

By *Prob.* III. the *Meaſure* of the Water paſſing through a Section of the Vein in the Time T is $\dfrac{2\,m\,\varrho^2\,A\,\upsilon}{V}$: But this is equal to the given *Meaſure* $2\,m\,r^2\,A\,q$; whence $\varrho^2\,\upsilon = r^2\,V\,q$.

Moreover, by the ſame *Prob.* III. the *Motion* of the Water paſſing through a Section of the Vein in the Time T is $\dfrac{2\,m\,r^2\,A\,\upsilon^2}{V}$, to which is equal the *Motion* determined by the former Problem, $3\,q^2\,m\,r^2\,A\,V$, wherefore $2\,\varrho^2\,\upsilon^2 = 3\,q^2\,r^2\,V^2$.

But $\upsilon = \dfrac{2\,\varrho^2\,\upsilon^2}{2\,\varrho^2\,\upsilon} = \dfrac{3\,q^2\,r^2\,V^2}{2\,q\,r^2\,V} = \dfrac{3\,q\,V}{2}$.

And $\varrho^2 = \dfrac{r^2\,V\,q}{\upsilon} = r^2\,V\,q \times \dfrac{2}{3\,q\,V} = \dfrac{2\,r^2}{3}$; wherefore $\varrho = \dfrac{r\sqrt{2}}{\sqrt{3}}$

Q. E. I.

Coroll. 1. The ſame Proportion remains between the *Radius* of the Hole, and the *Radius* of the contracted Vein, whether the *Motion* of the effluent Water be in any Manner diminiſhed by Reſiſtance, as in this Prob. or not diminiſhed, as in *Prob.* III. ſeeing it is either way $\varrho = \dfrac{r\sqrt{2}}{\sqrt{3}}$.

Coroll. 2. When the Motion of the effluent Water is diminiſhed by Reſiſtance, the Velocity is at the ſame Time diminiſhed in the contracted Vein. For when in *Prob.* III. it had been $\upsilon = \dfrac{V\sqrt{3}}{2}$, it now becomes $\upsilon = \dfrac{3\,q\,V}{2}$, that is, υ is diminiſhed from $V \times 0{,}866$ to $V \times 0{,}856$, taking $q = 0{,}571$, according to *Poleni*'s *Experiments*.

Schol. Accurately it will be $\upsilon = V \times r^2\,q \times \overline{\dfrac{L+K-\frac{3}{2}\,r^2}{M+N-4\,A\,r|^2}}$ & $\varrho = \sqrt{2} \times \dfrac{\overline{M+N-4\,A\,r}}{\sqrt{L+K-\frac{3}{2}\,r^2}}$, in like Manner as it was found in *Prob.* III.

Schol. 2. The

The Water running out through a circular Hole in the Middle of the Bottom of a cylindrical Vessel, when the Particles of Water, as they flow downwards within the Vessel, suffer so great a Resistance from a Want of Lubricity, that the Motion *of the Water is notably diminished thereby, and also the* Measure *of the effluent Water being given, to determine the* Motion *of the same, and the Velocity with which it goes out through the middle of the Hole.* Prob. VI.

Let the given *Measure* of the Water running out in the Time T be $2\,m\,r^2\,A\,q$, as in *Prob.* IV. and by help of the same Problem we shall have the *Motion* of the same $3\,q^2\,m\,r^2\,A\,V$, and the Velocity, with which it goes out through the Centre of the Hole, or $v = 3\,q\,V$. Q. E. I.

When q is given, v is as V, that is, as $\surd$ A. *Coroll.*

You will find these accurately determined in *Prob.* IV. *Schol.* *Schol.*

The Water running out into the Air, and neglecting the Acceleration of the Water without the Hole proceeding from Gravity, if any 2 of the 3 following are given, namely the Measure *of the effluent Water, the Velocity in the* Axis *of the contracted Vein, and the Diameter of the same Vein, to determine the remaining one.* *Prob.* VII.

When the Water bursting out of the Hole is carried through a *Vacuum*, it is shewn in the Solution of *Prob.* III. that the Velocity of the Particles of Water becomes equal through the whole Section of the contracted Vein: But now, when the Vein is carried through the Air, that Equality of Velocity must necessarily be taken away. For the outer Parts of the Vein stir the surrounding Air into Motion, and are retarded by it, so that they cannot acquire an equal Velocity with the rest. But the outer Parts, when they are retarded by the Air, retard the inner contiguous Parts, and they the next; and by this Means every inner Particle is carried swifter than the contiguous outer one, so that the Velocity is greatest in the Axis of the Vein, and least in the Circumference. And as the outer Parts are carried more slowly through the Air, than they would be carried through a *Vacuum*, it thence comes to pass, that the middle Parts are carried more swiftly, the Air surrounding the Vein, than they would be carried, on the Removal of the Air. For which Reason the middle Parts of the Water in Fountains rise much higher in the open Air, than they would rise *in Vacuo*, as we observed at the latter End of *Prob.* III. *Schol.* I.

Moreover, those Parts of the Air, which are contiguous to the Vein of Water, when they are stirred into Motion by the Water, stir others into Motion, that lie near them on the outside, and these the next outer ones, and those the rest successively to some certain Distance from the Circumference of the Vein.

But the Velocity of the Particles of Water, must necessarily so decrease from the *Axis* of the Vein to it's Circumference, that the relative Velocity of every Particle wheresoever situated, may be every

where

where one and the fame, with refpect to the Particle lying on the outfide, for the Caufes mentioned in the Solution of *Prob.* II. For if any Particle has a greater relative Velocity than the reft, it muft find a greater Refiftance from the Attrition of the adjacent Particle outwards, and by that means will be brought to an equal relative Velocity with the reft. In like Manner every Particle of the furrounding Air, which is ftirred into Motion, will have one, and the fame relative Velocity with Refpect to the adjacent Particle of Air outwards.

But the relative Velocity of the Particles of Water among themfelves, is very different from the relative Velocity of the Particles of Air, which may be conceived in this Manner.

Any Particle of Water in the outer Part of the Vein is follicited by the next Particle of Water inwards to accelerate the Motion; and is alfo retarded by the next Particle of Air: And when that outer Particle has acquired the due Velocity, thefe two contrary Forces muft needs be equal, one of which retards the Particle, and the other accelerates it. But that cannot be done, unlefs the *Factum* of the relative Velocity, and of the Denfity of the accelerating Particle of Water is equal to the *Factum* of the relative Velocity, and of the Denfity of the retarding Particle of Air. But the Denfity of Air, is to the Denfity of Water as 1 to 900 nearly. Therefore the relative Velocity between the outer Particle of Water, and the next of Air, is to the relative Velocity of the 2 next Particles of Water as 900 to 1 nearly.

Moreover, that inmoft Particle of Air is follicited by the next contiguous Particle of Water to accelerate the Motion, and retarded by the next Particle of Air outwards. And as here two contrary Forces are equal to one another, the *Factum* of the relative Velocity and Denfity of the accelerating Particle of Water, will be equal to the *Factum* of the relative Velocity and Denfity of the retarding Particle of Air. Wherefore the relative Velocity, which is between thofe 2 Particles of Air, will be to the relative Velocity, which is between the inmoft Particle of Air, and the next of Water, as 900 to 1 nearly; and it will be to the relative Velocity, which is between the 2 next Particles of Water, as 900×900 to 1 nearly: And this fo great relative Velocity will always be conftant through the whole Thicknefs of the Ring of Air, which is ftirred into Motion by the running Water.

Now let the fame be fignified by the Letters r, m, v, a, V, A, T, as in *Prob.* II. Alfo let υ be the Velocity of the Water in the *Axis* of the contracted Vein of Water, ϱ the *Radius* of the fame Vein, R the *Radius* of an imaginary Vein, by which the Velocity υ, by decreafing gradually, in like Manner as it decreafes in the true Vein, is reduced to nothing.

Alfo let the *Meafure* of the Water running out through the Hole in the Time T, be $2\,q\,m\,r^2\,A$.

Now

Now the *Measure* of the Water running in the same Time through the contracted Vein, by the Method laid down in *Prob.* II. will be found $\frac{2mA\upsilon\varrho^2}{3RV} \times \overline{3R - 2\varrho}$.

But these *Measures* are equal, that is, $2qmr^2A = \frac{2mA\upsilon\varrho^2}{3RV} \times \overline{3R - 2\varrho}$, or $3qr^2RV = \upsilon\varrho^2 \times \overline{3R - 2\varrho}$.

Moreover, as the *Measure* of the Water running through the Hole in the Time T, is $2qmr^2A$, the *Motion* of the same, by *Prob.* VI. is $3q^2mr^2AV$.

And the *Motion* of the Water running through the Vein in the same Time, by the Method used in *Prob.* II. is found $\frac{mA\upsilon^2 \times \overline{6R^2\varrho^2 - 8R\varrho^3 + 3\varrho^4}}{3VR^2}$.

Now these are equal, that is, $3q^2mr^2AV = \frac{mA\upsilon^2 \times \overline{6R^2\varrho^2 - 8R\varrho^3 + 3\varrho^4}}{3VR^2}$, or $9q^2r^2R^2V^2 = \upsilon^2 \times \overline{6R^2\varrho^2 - 8R\varrho^3 + 3\varrho^4}$.

These 2 Equations being rightly reduced, in order to extirpate R we come to the following Equation, $\varrho^4\upsilon^2 = 2q\upsilon Vr^2\varrho^2 + 12q^2V^2r^2\varrho^2 - 9q^2V^2r^4$, wherefore $\varrho^2 = \frac{qVr^2}{\upsilon^2} \times \overline{\upsilon + 6qV - 2\sqrt{3q\upsilon V + 9q^2V^2 - 2\upsilon^2}}$, and hence is obtained ϱ itself, or the *Radius* of the contracted Vein, seeing q and υ are given.

Moreover from the same Equation is drawn $\upsilon = \frac{qVr}{\varrho^2} \times \overline{r + 2\sqrt{3\varrho^2 - 2r^2}}$.

Lastly $q = \frac{\varrho^2\upsilon}{rV \times \overline{r + 2\sqrt{3\varrho^2 - 2r^2}}}$. Q. E. I.

We supposed above, that the *Motion* of the Water running through the contracted Vein is equal to the *Motion* of that which runs through the Hole. But this is not true in Mathematical Strictness. For the *Motion* of the Water running through the Hole is equal to the *Motion* of the Water running through the contracted Vein, and to the Motion of the Ring of Air surrounding the Vein, which Air is stirred into Motion by the Water running through the Vein, taken together. But we look upon the *Motion* of the Ring of Air as nothing, since it's Thickness is not greater than $\frac{R - \varrho}{900 \times 900}$, and it's Density is not greater than $\frac{1}{900}$ Part of the Density of the Water; and

Schol. II.

and by doing this we render the Equations far more ſimple than otherwiſe they would be.

Schol. II. By *Prob.* V. *Cor.* 1. when the Water runs out into a *Vacuum*, the ſame *Ratio* continues between the *Radius* of the Hole, and the *Radius* of the contracted Vein, whether the *Motion* of the effluent Water be in any Manner diminiſhed by Reſiſtance or not. Wherefore, as in a Phyſical Matter, we think it very near the Truth, that the *Ratio* between theſe *Radii* ſhould be conſidered as given, even when the Water runs through Air, howſoever the *Motion* of the effluent Water may be diminiſhed by Reſiſtance, or at leaſt, that this *Ratio* ſhould be changed as little as poſſible. And as this is found to agree with the Experiments hitherto made, as will more plainly appear below, we ſhall look upon it as true, till we ſhall be informed of ſomething more certain by more accurate Experiments.

Moreover, if a *Ratio* is given between r and ϱ, a *Ratio* is alſo given between r and R, or a *Ratio* between the *Radius* of the Hole, and the imaginary *Radius*, by which the Velocity v, by gradually decreaſing is reduced to nothing.

For by eliminating v from the 2 above Equations, $9 q^2 r^2 R^2 V_2 = \varrho^2 v^2 \times \overline{6 R^2 - 8 R \varrho + 3 \varrho^2}$, and $3 q r^2 R V = \varrho^2 v \times \overline{3 R - 2 \varrho}$, we come to the Equation $\varrho^2 \times \overline{9 R^2 - 12 R \varrho + 4 \varrho^2} = r^2 \times \overline{6 R^2 - 9 R \varrho + 3 \varrho^2}$, wherefore $R = \frac{\varrho}{3} \times 2 + \frac{r}{\sqrt{3 \varrho^2 - 2 r^2}}$.

Beſides, from one of theſe Equations, $3 q r^2 R V = \varrho^2 v \times \overline{3 R - 2 \varrho}$; we have $3 r^2 R : \varrho^2 \times \overline{3 R - 2 \varrho} :: v : q V$, and ſince the former *Ratio* is given, the latter *Ratio* is alſo given, that is, the Quantity $\frac{v}{q V}$ is given.

We ſhall afterwards demonſtrate, how great theſe 3 given *Ratios* are.

The ſame continued. No. 453. p. 65. Apr. &c. 1739. *Of the Reſiſtance of the Parts of Water among themſelves, proceeding from a Want of Lubricity.*

2. Before we proceed any farther, we muſt conſider that Reſiſtance of Fluids, which ariſes from the Motion of the ſame Parts among themſelves, and is called by Sir *I. Newton*, a *Reſiſtance* ariſing from a want of Lubricity.

He makes it of two Sorts, one ariſing from the Tenacity of the Fluid, the other from the mutual Attrition or Friction of the Parts of the Fluid between themſelves.

He thinks the Firſt is uniform in a given Surface, or that it produces an Effect proportional to the Time; which Opinion is favoured by Experiments: He is of Opinion that the latter is increaſed in Proportion to the Velocity, or in a Proportion ſomething leſs. But he does not determine any Thing about this, for want of ſuitable Experiments.

But theſe two Reſiſtances have a different Proportion between themſelves, not only according to the diverſity of the Fluid, as for Inſtance, there is a greater Tenacity and leſs Attrition in Oil or melted Suet than in Water; but alſo in the ſame Fluid, according to the different Velocity with which the parts of the Fluid are moved among themſelves. But in a given Fluid there muſt neceſſarily be ſome certain Velocity, where theſe Reſiſtances are equal between themſelves; and if we could find that Velocity by Experiment, their Proportion might be determined in any other Velocities. But we have no Experiments, that I know of, nor is it eaſy to contrive any, by means of which that Velocity may be known, which may ſerve for a Foundation to the reſt.

We ſuſpect indeed, nay we think it probable, that the very leaſt of that fundamental Velocity is not in Water from one Cauſe, where the Reſiſtances ariſing from Tenacity and Friction are equal between themſelves. But this being granted, when, as the Velocity increaſes, the Reſiſtance from Friction in like manner increaſes, but the Reſiſtance from Tenacity in no wiſe increaſes, it is plain, that this laſt Reſiſtance has but a very ſmall Proportion to the firſt, where the Parts of the Fluid are moved among themſelves with any notable Velocity; and therefore, that it may ſafely be neglected.

However, whether we neglect this, and take only the other Reſiſtance, which ariſes from Friction, or comprehend both under the name of *Reſiſtance* ariſing from want of Lubricity, certainly the Laws, by which this *Reſiſtance* increaſes or is diminiſhed, are to be ſought only from Experience. Therefore when we aſcribe to it the following laws of increaſing, tho' after a diligent Conſideration of the Experiments hitherto made, they may ſeem to have a great Probability, and this we do with an Intent, that if future Experiments ſhould teach any thing more certain, we may not unwillingly change our Minds.

The *Reſiſtance*, which ariſes from a want of Lubricity of the Water, is in a *Ratio* compounded of the 3 following: *Hypotheſis.*

1. Of the *Ratio* of the Surface of the Parts which are moved. This, I think, all Philoſophers admit.

2. Of the *Ratio* of the relative Velocity, by which the Parts of Water are moved among themſelves. This, if I miſtake not, is admitted by the reſt, Nor does Sir *I. Newton* much differ.

3. Of the ſubduplicate *Ratio* of the Altitude of the Water. For we aſſume this, being led by Experience, and in ſome Meaſure alſo by Sir *I. Newton*, who thinks that the Attrition of the Parts becomes ſtronger, and their Separation from each other more difficult by greater Preſſure *.

To explain the Reſiſtance of the Parts of the Cataract, *which ariſes from a want of Lubricity.* Prob. VIII.

* *Princip.* Lib. II. Prop. lii. Schol.

Let r be the *Radius* of the Hole, A the Altitude of the *Cataract*, y the *Radius* of any horizontal Section, x the Altitude of the *Cataract* above that Section, z the *Radius* of any Circle in that Section, v the Velocity of the Water in the Centre of the Hole.

Now $\frac{v x^{\frac{1}{2}}}{A^{\frac{1}{2}}}$ will be the Velocity of the Water in the Centre of the Section, whose *Radius* is y. For the Velocity in the Centre of the Section, is the same as if that Section was a Hole in the Bottom of a shortened Vessel, whose Altitude is x; and therefore is as $x^{\frac{1}{2}}$ by *Prob.* VI. *Coroll.* Also $\frac{y-z}{y} \times \frac{v x^{\frac{1}{2}}}{A^{\frac{1}{2}}}$ will be the Velocity of the Water in the Circumference of the Circle, whose *Radius* is z; $\frac{\dot{z} v x^{\frac{1}{2}}}{y A^{\frac{1}{2}}}$ the relative Velocity $2\, m z \dot{x}$ the Surface of the nascent Cylinder, whose *Radius* is z and Altitude $\dot{x}$, and by our 3 Positions the *Resistance* of the Surface of this Cylinder, as $2\, m z \dot{x} \times \frac{\dot{z} v x^{\frac{1}{2}}}{y A^{\frac{1}{2}}} \times x^{\frac{1}{2}} = \frac{2\, m v \dot{x} x z \dot{z}}{y A^{\frac{1}{2}}}$.

Now let $\dot{x}$, x, and y be considered as constant Quantities, whilst z flows till it becomes equal to y; and the fluent Quantity of the Fluxion $\frac{2\, m v \dot{x} x z \dot{z}}{y A^{\frac{1}{2}}}$ will be $\frac{2\, m v \dot{x} x z^2}{2\, y A^{\frac{1}{2}}}$, or $\frac{m v \dot{x} x z^2}{y A_{2}}$, or (making $z = y$) $\frac{m v \dot{x} x y}{A^{\frac{1}{2}}}$, as the Resistance of the nascent Cylinder, whose *Radius* is y, and Altitude $\dot{x}$.

But by the property of the *cataractic* Curve $y^4 x = r^4 A$, and $y x^{\frac{1}{4}} = r A^{\frac{1}{4}}$: Whence the *Resistance* of this nascent Cylinder will be as $\frac{m v \dot{x} x r A^{\frac{1}{4}}}{A^{\frac{1}{2}} x^{\frac{1}{4}}}$, or as $\frac{m v r \dot{x} x^{\frac{3}{4}}}{A^{\frac{1}{4}}}$, and the *Resistance* of the whole *Cataract* will be as the fluent Quantity of this Fluxion, or as $\frac{m v r x^{\frac{7}{4}}}{A^{\frac{1}{4}}}$

$\times \frac{4}{7}$, or, making $x = A$ as $\frac{4}{7}\, m\, v\, r\, A^{\frac{3}{2}}$. And since by *Prob.* IV

$v = 3\, q\, V$, the *Resistance* in the *Cataract* will be, as $\frac{12\, q\, m\, V\, r\, A^{\frac{3}{2}}}{7}$,

or as $q\, V\, r\, A^{\frac{3}{2}}$. Q. E. I.

Since V is as $\sqrt{A}$, the *Resistance* in the *Cataract* will be as $q\, r\, A^2$. *Coroll.*

In the Solution just now made, instead of the Surface of the *cataractic* *Taleola*, whose *Radius* is z, according to which the Particles of Water pass by each other with an equable relative Velocity, we made use of the Surface of the nascent Cylinder, whose *Radius* is z, and Altitude $\dot{x}$, or of the Surface $2\, m\, z\, \dot{x}$, when really the Surface of that *Taleola* is $2\, m\, z \times \sqrt{\dot{x}^2 + \dot{z}^2}$. *Schol.*

But if that is corrected, the *Resistance* of the Surface of this *Taleola* will be found as $2\, m\, z\, \sqrt{\dot{x}^2 + \dot{z}^2} \times x^{\frac{1}{2}} \times \frac{z\, v\, x^{\frac{1}{2}}}{y\, A^{\frac{1}{2}}}$

$$= \frac{2\, m\, v\, x\, \dot{z}\, z\, \sqrt{\dot{x}^2 + \dot{z}^2}}{y\, A^{\frac{1}{2}}}$$

And since by *Prob.* II. *Schol.* 2. the Subtangent of the *cataractic* Curve is $4\, x$, and the Tangent itself $\sqrt{16 x^2 + z^2}$, therefore $4\, x : \sqrt{16 x^2 + z^2} :: \dot{x} : \sqrt{\dot{x}^2 + \dot{z}^2} = \frac{\dot{x}\, \sqrt{16\, x^2 + z^2}}{4\, x}$.

Therefore the *Resistance* of the Surface of the *Taleola* will be as

$$\frac{2\, m\, v\, x\, z\, z}{y\, A^{\frac{1}{2}}} \times \frac{\dot{x}}{4x} \sqrt{16\, x^2 + z^2} = \frac{m\, v\, \dot{x}}{2\, y\, A^{\frac{1}{2}}}\, z\, z\, \sqrt{16\, x^2 + z^2}$$

$$= \frac{m\, v\, \dot{x}\, \dot{z}\, z}{2\, y\, A^{\frac{1}{2}}} \text{ in } 4\, x + \frac{z^2}{2 \times 4\, x} - \frac{z^4}{8 . 4\, x^3} + \frac{z^6}{16 \times 4\, x^5} - \frac{5\, z^8}{128 \times 4 x^7}$$

$$+ \frac{7\, z^{10}}{256 \times 4\, x^9} \text{ \&c.} = \frac{m\, v\, \dot{x}}{2\, y\, A^{\frac{1}{2}}} \text{ in } 4\, x\, \dot{z}\, z + \frac{\dot{z}\, z^3}{2 \times 4\, x} - \frac{\dot{z}\, z^5}{8 \times 4\, x^3}$$

$$+ \frac{\dot{z}\, z^7}{16 \times 4\, x^5} - \frac{5\, \dot{z}\, z^9}{128 \times 4\, x^7} + \frac{7\, \dot{z}\, z^{11}}{256 \times 4\, x^9} - \frac{21\, \dot{z}\, z^{13}}{1024 \times 4\, x^{11}} \text{ \&c.}$$

 But

But by taking the Quantities $\dot{x}$, x, and y for constant ones, the Fluent of this Fluxion will be $\frac{m v \dot{x}}{2 y A^{\frac{1}{2}}}$ in $\frac{4 x z^2}{2} + \frac{z^4}{8 \times 4 x} - \frac{z^6}{48 \times \overline{4 x}^3} + \frac{z^8}{8 \times 16 \times \overline{4 x}^5} - \frac{z^{10}}{256 \times \overline{4 x}^7} + \frac{7 z^{12}}{12 \times 256 \times \overline{4 x}^9}$ — &c.

And by supposing $z = y$, this Fluent will be $\frac{m v \dot{x}}{2 A^{\frac{1}{2}}}$ in $2 x y + \frac{y^3}{8 \times 4 x} - \frac{y^5}{48 \times \overline{4 x}^3} + \frac{y^7}{8 \times 16 \times \overline{4 x}^5} - \frac{y^9}{256 \times \overline{4 x}^7} + \frac{7 y^{11}}{12 \times 256 \times \overline{4 x}^9}$ — &c. which will be as the *Resistance* in the cataractic *Taleola*, whose *Radius* is y, and Altitude $\dot{x}$.

But this is as the Fluxion of the *Resistance* in the whole *Cataract*, and by putting $y = \frac{r A^{\frac{1}{4}}}{x^{\frac{1}{4}}}$, it becomes $\frac{m v \dot{x}}{2 A^{\frac{1}{2}}}$ in $\frac{2 r x A^{\frac{1}{4}}}{x^{\frac{1}{4}}} + \frac{r^3 A^{\frac{3}{4}}}{8 \times 4 \times x^{\frac{7}{4}}}$

$$- \frac{r^5 A^{\frac{5}{4}}}{48 \times 4^3 \times x^{\frac{17}{4}}} + \frac{r^7 A^{\frac{7}{4}}}{8 \times 16 \times 4^5 \times x^{\frac{27}{4}}} - \frac{r^9 A^{\frac{9}{4}}}{256 \times 4^7 \times x^{\frac{37}{4}}} + \text{\&c.}$$

$$= \frac{m v r}{2 A^{\frac{1}{4}}} \text{ in } 2 \dot{x} x^{\frac{3}{4}} + \frac{r^2 A^{\frac{1}{2}} \dot{x} x^{-\frac{7}{4}}}{32} - \frac{r^4 A \dot{x} x^{-\frac{17}{4}}}{48 \times 4^3} + \frac{r^6 A^{\frac{3}{2}} \dot{x} x^{-\frac{27}{4}}}{8 \times 16 \times 4^5}$$

— &c. But the fluent Quantity of this Fluxion is $\frac{m v r}{2 A^{\frac{1}{4}}}$ in $2 x^{\frac{7}{4}} \times \frac{4}{7}$

$$+ \frac{r^2 A^{\frac{1}{2}} x^{-\frac{3}{4}}}{32} \times \frac{4}{3} - \frac{r^4 A x^{-\frac{13}{4}}}{48 \times 4^3} \times \frac{4}{13} + \frac{r^6 A^{\frac{3}{2}} x^{-\frac{23}{4}}}{8 \times 16 \times 4^5} \times - \frac{4}{23}$$

— &c. And this, supposing $x = A$, becomes $\frac{m v r}{2}$ in $\frac{8 A^{\frac{2}{2}}}{7}$

$$- \frac{r^2}{3 \times 8 A^{\frac{1}{2}}} + \frac{r^4}{12 \times 13 \times 4^3 A^{\frac{5}{2}}} - \frac{r^6}{32 \times 23 \times 4^5 A^{\frac{9}{2}}} + \text{\&c.}$$

or

or $\frac{4\,m\,v\,r\,A^{\frac{3}{2}}}{7}$ in $1 - \frac{7\,r^2}{3 \times 4^3\,A^2} + \frac{7\,r^4}{6 \times 13 \times 4^5\,A^4} - \frac{7\,r^6}{23 \times 4^9\,A^6}$ + &c. which is as the *Resistance* thro' the whole *Cataract.*

But if the Altitude be accounted as infinite with respect to the Diameter of the Hole, the *Resistance* will be as $\frac{4\,m\,v\,r\,A^{\frac{3}{2}}}{7}$ as was determined in the former Solution.

If $A = 10\,r$, the *Resistance* will be as $\frac{4\,m\,v\,r\,A^{\frac{3}{2}}}{7} \times \overline{1 - \frac{1}{2743}}$ nearly.

If $A = 4\,r$, the *Resistance* will be as $\frac{4\,m\,v\,r\,A^{\frac{3}{2}}}{7} \times \overline{1 - \frac{1}{439}}$ nearly.

We may therefore use $\frac{4\,m\,v\,r\,A^{\frac{3}{2}}}{7}$ for the Measure of the Resistance, without Danger of any sensible Error, even when the Altitude of the Water does not exceed two Diameters of the Hole, and much more in a far greater Altitude.

The Measure *being given of the Water running out thro' a given circular Hole in the Middle Part of the Bottom of a Cylindrical Vessel of a given Depth, to determine the* Measure *of the Water running out of another Vessel of any given Depth, thro' any given circular Hole whatsoever.* Prob. IX.

Let r be the *Radius* of the given Hole, A the given Depth, $2\,q\,m\,r^2\,A$ the given Measure of the Water running out in that Time, in which a heavy Body would fall *in Vacuo* thro' the Altitude A.

Hence by *Prob.* IV. $3\,q^2\,m\,r^2\,A\,V$ will be the *Motion* of the Water running out in the same Time: and by *Prob.* IV. *Cor.* the *Motion* lost in the same Time by the Resistance will be $m\,r^2\,A\,V \times \overline{1 - 3\,q^2}$. Therefore the equal Force of Resistance can generate this *Motion* in the same Time.

But the *Motions* are generated in the same Space of Time with Forces generating the same proportional.

Therefore the *Motion* $m\,r^2\,A\,V$, which the Weight of the Column of Water $m\,r^2\,A$ can generate in this Time, by *Prob.* 1. when all *Resistance* is away, is to the *Motion* $m\,r^2\,A\,V \times \overline{1 - 3\,q^2}$, which the *Resistance*

can

can generate in the same Time, as the Weight $m r^2 A$ to the *Resistance* itself. Wherefore the Resistance $= m r^2 A \times \frac{m r^2 A V \times \overline{1 - 3q^2}}{m r^2 A V} = m r^2 A \times \overline{1 - 3 q^2}$.

After the same manner, by putting s and E for the *Radius* of the Hole, and the Altitude of the new Vessel, and $2\,p\,m\,s^2$ E for the Measure of the Water running out in the same Time, in which a heavy Body would fall *in Vacuo* thro' the Altitude E, you will have the *Resistance* in the new Vessel $= m s^2 E \times \overline{1 - 3 p^2}$.

But by *Prob.* VIII. *Cor.* these 2 *Resistances* are to each other as $q r A^2$ to $p s E^2$.

Therefore $m r^2 A \times \overline{1 - 3 q^2} : m s^2 E \times \overline{1 - 3p^2} :: q r A^2 : p s E^2$, or $r \times \overline{1 - 3 q^2} : s \times \overline{1 - 3 p^2} :: q A : p E$, or $p r E \times \overline{1 - 3 q^2} = q s A \times \overline{1 - 3p^2}$, which Equation being rightly reduced we come to the following, $p = \sqrt{\frac{1}{3} + \left(\frac{r E \times \overline{1 - 3 q^2}}{6 q s A}\right)^2} - \frac{r E \times \overline{1 - 3 q^2}}{6 q s A}$, or making $r E = n s A$.

$$p = \sqrt{\frac{1}{3} + \left(\frac{n \times \overline{1 - 3 q^2}}{6 q}\right)^2} - \frac{n \times \overline{1 - 3 q^2}}{6 q}.$$

Whence we have $p \times 2 m s^2 E$, which is the *Measure* of the Water running out of the second Vessel, in the Time that a heavy Body falls *in Vacuo* thro' the Altitude E. Q. E. I.

Coroll. 1. If the Diameters of the Holes shall be in a *Ratio* of the Altitudes of the Water, the *Ratio* of the *Measures* will be the same, as if the Water ran out without any *Resistance.*

For if $r : s :: A : E$, $r E = s A$, and $n = 1$, wherefore $p = \sqrt{\frac{1}{3} + \left(\frac{1 - 3 q^2}{6 q}\right)^2} - \frac{1 - 3 q^2}{6q}$, and by Reduction $p = q$; wherefore $2 q m r^2 A : 2 p m s^2 E :: 2 m r^2 A : 2 m s^2 E$, which is the *Ratio* of the *Measures*, when all *Resistance* is away.

Coroll. 2. If E is accounted for nothing with Regard to the Altitude A, then n also must be accounted for nothing, whence $p = \frac{1}{\sqrt{3}}$. Therefore, the smaller the Altitude E is taken, the nearer p comes to $\frac{1}{\sqrt{3}}$.

If

If s is to be accounted infinitely great with reſpect to the *Radius* r, then $p = \frac{1}{\sqrt{3}}$. Therefore the greater the *Radius* s is taken, the more p verges to $\frac{1}{\sqrt{3}}$. *Coroll.* 3.

The Water running out into the Air, to determine the Proportion between the Diameter of the Hole and the Diameter of the contracted Vein. Prob. X.

This Proportion cannot be determined without the help of Experiments. By *Prob.* VII. $\varrho^2 = \frac{qVr^2}{\upsilon^2} \times \upsilon + 6qV - 2\sqrt{3q\upsilon V + 9q^2V^2 - 2\upsilon^2}$, whence q and υ being known ϱ is determined.

But we have no Experiments, that I know of, by which we may meaſure q and υ.

Poleni's Experiments exhibit the *Meaſure* of the effluent Water whence q is known; but they do not ſhew the greateſt Diſtance, to which the Water is carried that comes horizontally out of the Hole, or the Diſtance to which the middle Part of the Vein reaches, that comes out with the Velocity υ.

But *Mariotte*'s Experiments meaſure the greateſt perpendicular Height, to which Water riſes, when it's Motion is turned upwards, or the Height, which the Water coming out from the middle of the Vein reaches, whence υ^2 is known; but they do not exhibit the *Meaſure* of the effluent Water.

Therefore for want of fit Experiments, we ſhall hardly be able to determine the Proportion ſought any otherwiſe than probably; and this we ſhall do in the following manner.

In *Prob.* VII. *Schol.* 2. we ſhewed it to be probable, that the *Ratio* is conſtant between theſe *Radii*, or at leaſt that it is very little changed.

It is manifeſt from *Mariotte*'s Experiments, that the difference between the Altitude, which the Water ſpringing upwards reaches, and the Altitude of the Veſſel, has nearly a duplicate *Ratio* of the Altitude of the Veſſel.

Therefore let a be the Height, to which the Water running thro' the *Axis* of the Vein with the Velocity υ can riſe; then, by *Mariotte*'s Experiments, $A - a$ as A^2, and $\frac{A^2}{A - a}$ will be the given Quantity.

But in one Experiment, which *Mariotte* reckons a fundamental one, A was $= 60$ *Paris* Inches, and he found $a = 59$ *Paris* Inches, the Diameter of the Hole meaſuring $\frac{1}{2}$ an Inch. Therefore in this caſe $\frac{A^2}{A - a} = 3600$, and as this Quantity is given, it will always be $3600\, a = 3600\, A - A^2$, or $a = \frac{3600\, A - A^2}{3600} = A - \frac{A^2}{3600}$.

Therefore

Therefore if A = 1 Inch, or is double the Diameter of the Hole, $a = 1 - \frac{1}{3600}$. But $v^2 : V^2 :: a : A :: 1 - \frac{1}{3600} : 1$.

Therefore as the Altitude of the Veſſel is double the Diameter of the Hole, we may have $v^2 = V^2$, or $v = V$.

Moreover, by *Prob.* IX. *Cor.* 4. E decreaſing, p tends to $\frac{1}{\sqrt{3}}$.

Therefore when the Altitude of the Veſſel is very ſmall, as if it does not exceed 2 Diameters of the Hole, we may have p or $q = \frac{1}{\sqrt{3}}$.

But by *Prob.* VII.

$\varrho^2 = \frac{q V r^2}{v^2} \times \overline{v + 6 q V - 2\sqrt{3 q v V + 9 q^2 V^2} - 2 v^2}$, and ſubſtituting inſtead of v and q the Values of the ſame juſt now found, or V and $\frac{1}{\sqrt{3}}$, it becomes $\varrho^2 = \frac{r^2}{V\sqrt{3}} \times \overline{V + 2V\sqrt{3} - 2\sqrt{V^2\sqrt{3} + 3V^2} - 2V^2}$ $= \frac{r^2}{\sqrt{3}} \times \overline{1 + 2\sqrt{3} - 2\sqrt{1 + \sqrt{3}}}$, or $\varrho^2 = r^2 \times 2 + \frac{1}{\sqrt{3}} - 2\sqrt{1 + \sqrt{3}} = r^2 \times 0{,}6687553907$ whence $\varrho = r \times 0{,}81777466$.

Here therefore is the Value of ϱ, when the Altitude of the Water is double the Diameter of the Hole; and as by *Prob.* VII. *Schol.* 2. ϱ obtains a conſtant Proportion to the *Radius* of the Hole, it will obtain the ſame Value in any Altitude of Water. Q. E. I.

Coroll. 1. By *Prob.* VII. $R = \frac{\varrho}{3} \times \frac{\overline{2 \times r}}{\sqrt{3\varrho^2 - 2r^2}}$, and by the Value of ϱ juſt now found, we have $R = r \times 3{,}98877150$, which is the Value of R, when the Altitude of the Water is double the Diameter of the Hole; and as by *Schol.* 2. of the ſame *Prob.* there is a conſtant Proportion between r and R, therefore R will obtain this very Value, whatſoever may be the Altitude of the Water.

Coroll. 2. Becauſe v is almoſt $= V$, and q is almoſt $= \frac{1}{\sqrt{3}}$, when the Altitude of the Water is double the Diameter of the Hole, therefore it will be to this Altitude of the Water $\frac{v}{qV} = \sqrt{3}$ very nearly. And as by *Prob.* VII. *Schol.* 2. the Proportion is conſtant between v and q V, therefore $\frac{v}{qV} = \sqrt{3}$, whatſoever the Altitude of the Water may be.

The

The Water running out of a given Vessel always full, thro' a given Hole, into the Air, and any one of the 3 following Quantities being given, namely, the Measure *of the effluent Water, the* Velocity *in the* Axis *of the contracted Vein, or the* Altitude, *to which the middle Part of the Vein can rise, the Motion being turned upwards, to determine the rest.* Prob. XI.

Let A be the Altitude of the Vessel, r the *Radius* of the Hole, $2\,q\,m\,r^2$ A, the measure of the effluent Water, v the Velocity in the *Axis* of the contracted Vein, a the Altitude, to which the Water running out thro' the *Axis* of the Vein can rise, and first let $2\,q\,m\,r^2$ be given, whence q is given.

By *Prob.* X. *Cor.* 2. $\frac{v}{q\,V} = \sqrt{3}$, whence $v = q\,V\sqrt{3}$. Hence $v^2 = 3\,q^2\,V^2$.

But $V^2 : v^2 :: A : a = \frac{v^2\,A}{V^2} = \frac{3\,q^2\,V^2\,A}{V^2} = 3\,q^2\,A$.

If, secondly, v is given, then $q = \frac{v}{V\sqrt{3}}$, and $2\,q\,m\,r^2\,A = \frac{2\,m\,r^2\,A^2}{V\sqrt{3}}$.

Moreover $a = \frac{v^2\,A}{V^2}$.

Lastly, if a is given, since $a = 3\,q^2\,A$, therefore $q^2 = \frac{a}{3\,A}$ and $q = \sqrt{\frac{a}{3\,A}}$.

Also $v^2 = \frac{a\,V^2}{A}$, whence $v = V\sqrt{\frac{A}{a}}$. Q. E. I.

The Altitude being given, to which, when the Motion is turned upwards, Water rises issuing thro' the Air from a Vessel of a given Altitude thro' a given circular Hole, to determine the Altitude, to which, when the Motion is turned upwards, Water will rise, when it issues from a Vessel of any given Altitude, thro' any given circular Hole. Prob. XII.

Let the letters r, s, A, E, q, p, express the same as in *Prob.* IX; and let a and e be the Altitudes to which Water can rise, issuing out of Vessels, the Altitudes of which are A and E respectively.

Now by *Prob.* XI. $a = 3\,q^2\,A$, $e = 3\,p^2\,E$, whence $3\,q^2 = \frac{a}{A}$, $1 - 3\,q^2 = \frac{A - a}{A}$, $q = \sqrt{\frac{a}{3\,A}}$, $p = \sqrt{\frac{e}{3\,E}}$, and $p^2 = \frac{e}{3\,E}$.

And since by *Prob.* IX. $p = \sqrt{\frac{1}{3} + \left|\frac{rE \times \overline{1-3q^2}}{6qsA}\right|^2} - \frac{rE\times\overline{1-3q^2}}{6qsA}$,

or making $rE = nsA$, $p = \sqrt{\frac{1}{2} + \left|n \times \frac{1-3q^2}{6q}\right|^2} - n \times \frac{1-3q^2}{6q}$;

hence by substituting $\frac{A-a}{A}$ for $1-3q^2$, and $\sqrt{\frac{a}{3A}}$ for q, and writing α for $A-a$, it will be $p = \frac{\sqrt{4Aa+n^2\alpha^2} - n\alpha}{2\sqrt{3Aa}}$,

and $p^2 = \frac{2Aa + n^2\alpha^2 - n\alpha\sqrt{4Aa+n^2\alpha^2}}{6A\alpha}$.

But $p^2 = \frac{e}{3E}$, whence $\frac{e}{E} = \frac{2Aa + n^2\alpha^2 - n\alpha\sqrt{4Aa+n^2\alpha^2}}{2A\alpha}$,

or $e = E \times \frac{2Aa + n^2\alpha^2 - n\alpha\sqrt{4Aa+n^2\alpha^2}}{2Aa}$, whence by writing ε for $E-e$ it becomes $\varepsilon = \frac{nE\alpha}{2Aa} \times \overline{\sqrt{4Aa+n^2\alpha^2} - n\alpha}$. Now ε or $E-e$ being given, e also is given, or the Altitude to which the Water is carried, when it issues out of the new Vessel.

Coroll. 1. If the Holes in both Vessels shall be equal, or $s=r$, then $E=nA$, or $n = \frac{E}{A}$, whence $\varepsilon = \frac{n^2\alpha}{2\alpha} \times \overline{\sqrt{4Aa+n^2\alpha^2} - n\alpha}$.

Coroll. 2. If the Altitudes of the Vessels shall be equal, or $E=A$, then $r=ns$, or $n=\frac{r}{s}$, whence $\varepsilon = \frac{n\alpha}{2a} \times \overline{\sqrt{4Aa+n^2\alpha^2} - n\alpha}$.

Coroll. 3. If the Diameters of the Holes shall be in a *Ratio* of the Altitudes, the Waters will spout to Altitudes proportional to the Altitudes of the Vessels. For if $r:s::A:E$, $re=sA$, and $n=1$, whence $\varepsilon = \frac{E\alpha}{A}$, or $\varepsilon:\alpha::E:A$, or $E-e:A-\alpha::E:A$, or $e:a::E:A$.

Coroll. 4. Since $p \times 2\sqrt{3Aa} = \sqrt{4Aa+n^2\alpha^2} - n\alpha$, therefore $\varepsilon = \frac{nE\alpha}{2Aa} \times 2p\sqrt{3Aa} = \frac{pnE\alpha\sqrt{3}}{\sqrt{Aa}}$, whence by substituting for $\sqrt{a}$ it's above-

above-mentioned Value $q\sqrt{3\,A}$, and by a due Reduction, ε becomes $= \frac{p\,n\,E\,\alpha}{q\,A}$, or $\varepsilon = \frac{p\,r\,E^2\,\alpha}{q\,s\,A^2}$.

Hence, by making $p = q$, $\varepsilon = \frac{r\,E^2\,\alpha}{s\,A^2}$, or $\varepsilon : \alpha :: r\,E^2 : s\,A^2$. *Coroll.* 5.

That is, the Defects of spouting Waters, or the Differences between the Altitudes of the Spouts, and the Altitudes of the Vessels are in a *Ratio* compounded of the duplicate *Ratio* of the Altitudes of the Vessels directly, and of the *Ratio* of the Diameters of the Holes reciprocally. And this Rule is exactly true, when $s\,A = r\,E$ by *Prob.* IX. *Cor.* 1. and comes very near the Truth, when E and s are increased or diminished in the same Proportion nearly; and it errs but little from the true Altitude of the salient Water in any case, provided E be not greater than 50 Feet, and at the same Time s be not less than 3 Lines.

When $s = r$, $\varepsilon = \frac{E^2\,\alpha}{A^2}$ nearly, that is, when the Holes are equal, *Coroll.* 6. the Defects of the Altitudes of spouting Waters are almost in a duplicate *Ratio* of the Altitudes of the Vessels, which is the very Rule of *Mariotte*.

When $E = A$, $\varepsilon = \frac{r\,\alpha}{s}$ nearly, that is, when the Altitudes of the *Coroll.* 7. Vessels are equal the Defects of the spouting Waters are almost as the Diameters of the Holes reciprocally.

First General Scholium.

If any one has a mind to examine the Truth of this Theory by Experiments, I would desire him,

1. To use a Vessel that is very large, at least in the upper Part, that, during the whole Time of making the Experiment, the Altitude of the Water may not sensibly be changed. But if the Vessel is not so large, but that during the Efflux from the Hole, a remarkable Decrease of the Water is found, then the just intermediate Altitude between the greatest and the least Altitude of the Water is to be taken for the constant Altitude; which is better than disturbing the natural Motion of the Water, by pouring fresh Water upon it.

2. Let the Vessel be of such a Depth, that if you would let out the Water thro' a Hole made in the Side, the Velocity of the Water going out thro' the Centre of the Hole may be safely taken for any Velocity, with which the Water will issue thro' all the Hole, when there is no Resistance.

3. Let the *Lamina*, in which the Hole is made, be so thin, or at least have so thin an Edge in the Circumference of the Hole, that the Thickness of that Edge may be accounted as nothing with respect to the Diameter of the Hole. But the Thickness of the *Lamina* should be shaved on the outer Face of the *Lamina*, leaving the inner Face next the Water plain;

plain; and the Angle of this Edge ſhould be ſo acute, that the Water iſſuing thro' the Hole may not adhere to the outer Side of the *Lamina.*

Theſe things being prepared, the following Experiments may be made, by which, as by ſo many *Criteria*, we may judge of the Certainty of the above Doctrine.

Exp. 1. When the Water is let out thro' a Hole in the Side of the Veſſel, let the Diameter of the contracted Vein be meaſured very diligently, obſerving whether it remains always the ſame; howſoever the Altitude of the Water may be changed.

Exp. 2. Let it be obſerved, whether this Diameter has always the ſame Proportion to the Diameter of the Hole, when Holes of different Magnitudes are uſed.

Exp. 3. The Water iſſuing, either ſtrait down thro' the Bottom of the Veſſel, or horizontally thro' it's Side, let it be very carefully obſerved how much runs out in a given Time, uſing different Altitudes of Water, but one and the ſame Hole.

Exp. 4. Let the ſame be obſerved, when Holes of a different Magnitude are uſed, but keep the ſame Depth of Water.

Exp. 5. Obſerve how much runs out in a given Time, in 2 different Caſes, in each of which there is the ſame Proportion of the Diameter of the Hole to the Altitude of the Water. For if the *Meaſures* ſhall be found in a *Ratio* compounded of a duplicate *Ratio* of the Diameters, and a ſimple *Ratio* of the Altitudes, as in *Prob.* IX. *Cor.* 3. you will have a great Confirmation of our Theory.

Exp. 6. In the ſame 2 Caſes, the Motion of the Water being turned upwards, by means of a large Tube fitted to the Side of the Veſſel, and perforated at the upper Part, obſerve to what Altitudes the Water will riſe. For if theſe Altitudes are found proportional to the Altitudes of the Water in the Veſſel, as in *Prob.* XII. *Cor.* 3. you will have another moſt certain Confirmation of this Theory.

Exp. 7. The ſame Hole continuing, but the Height of the Water being changed, obſerve to what Height the Water is carried.

Exp. 8. Let the ſame be obſerved, when the Magnitude of the Hole is changed, the Height of the Water continuing the ſame.

But of all theſe Experiments thoſe are to be preferred, by which the Height, to which the Water riſes, is noted, when the Motion of the Water is turned upwards. For this Height may far more eaſily be taken, than the *Meaſure* of the running Water, and the Error, if there is any, in taking the Altitude, is of far leſs Moment, than that which is committed in eſtimating the *Meaſure*. For as by *Prob.* XI. the Altitude of ſalient Water is $3\,q^2 A$, it is plain that the leaſt Error admitted in the *Meaſure*, or in q, will be almoſt doubled in q^2, and ſo it will be doubled in the Altitude of the ſalient Water.

But the leaſt Error admitted in the Altitude of the ſalient Water, or in $3\,q^2 A$, is reduced to almoſt half in eſtimating q, that is in the Meaſure of the effluent Water.

In

In the mean Time, till those Experiments are made by such Persons as have Leisure, as well as a Desire of knowing the Truth, we must use, as far as we can, those Experiments, with which we have been furnished by the Diligence of our Predecessors. *Second General Scholium.*

These are of 3 kinds: For they measure either,

1. The Diameter of the contracted Vein; or
2. The *Measure* of the effluent Water; or
3. The Altitude to which the Water rises.

1. The *Radius* of the contracted Vein, as measured by Sir *I. Newton*, $r \times 0{,}84$, when the Diameter of the Hole is $\frac{5}{8}$ of a *London* Inch.

The same, as measured by *Poleni*, is $r \times 0{,}78$ nearly, when the Diameter of the Hole is $2\frac{1}{8}$ *Paris* Inches.

By our Calculation it is $r \times 0{,}818$ nearly, whatsoever is the Diameter of the Hole, which is about the intermediate Magnitude between the Measures of *Newton* and *Poleni*.

2. It happens very unluckily, that none of the *Measures* of effluent Water, except those taken by *Poleni* are of any Use to our Purpose. For as he informs us, this *Measure*, when the Water issues thro' a Tube, is far greater than when it issues from a naked Hole. And as Holes made in *Laminæ* are to be looked upon as short Tubes, at least if the Thickness of the *Laminæ* is not as small as possible with respect to the Diameter of the Hole, and thence it comes to pass, that all the *Measures* of effluent Water taken before him are found to be greater than the Truth.

Therefore we must use only the Measures taken by *Poleni*. And these*, which were taken with that great Hole of 26 Lines, are 10 in Number, namely by supposing a heavy Body to fall *in Vacuo* thro' 15 Feet, 1 Inch, 10 Lines *Paris* Measure, in 1'', the Measure is

1	$= 2\,m\,r^2$	A	$\times$		0,5772
2	—	—	—		0,5772
3	—	—	—		0,5731
4	—	—	—		0,5710
5	—	—	—		0,5690
6	—	—	—		0,5675
7	—	—	—		0,5689
8	—	—	—		0,5703
9	—	—	—		0,5732
10	—	—	—		0,5613
					5,7087

Of all which the intermediate is $2\,m\,r^2\,A \times 0{,}571$ nearly. Therefore we have this for *Poleni*'s *Measure* of effluent Water, when the Altitude of the Vessel is 33 *Paris* Inches, which is the intermediate Altitude between those which were used by *Poleni*.

But the *Measure*, which is taken to this Altitude by our Calculation from *Mariotte*'s fundamental Experiment, which we shall produce

* *Polenius de Castellis*, Art. 35, 38, 39, 42, 43; & *Epist. ad Marinonium.*

presently

presently, is $2\ m\ r^2\ A \times 0,5768$, which exceeds *Poleni*'s Measure about $\frac{1}{78}$ Part. But so small a Difference might arise either from an Error of $\frac{1}{100}$ Part of an Inch in estimating the Diameter of the Hole; or from the Vessel that receives the effluent Water being about $\frac{1}{100}$ Part greater than in *Poleni*'s Computation; or partly from both. Add, that this Difference is twice as little as what is found between *Poleni*'s own Experiments.

3. We shewed before, that *Poleni* has rendered all the Experiments of his Predecessors useless concerning the Measure of effluent Water, because they took no Account of the Thickness of the *Lamina*, thro' which the Water issued. Whence some may not unreasonably suspect, that there is the same Fault in those Experiments, by which the Height of the salient Water was discovered. But *Poleni* has removed this Doubt by another excellent Observation. For he discovered the Measure of the Water to be greater in flowing from a Tube than from a naked Hole; but, what is wonderful, that Water issuing thro' Tubes * of 7 or 13 *Paris* Lines in Length, reaches only to the same, or very little less horizontal Distance, than it does when it issues from a naked Hole. Therefore the greatest Velocity of Water is very little less after it's Exit from a Tube, than after it's Exit from a Hole, when the Tube is not very short: but when the Tube is very short, such as a Hole in a *Lamina* that is not very thin, the greatest Velocity of the Water may be accounted the same after it's Exit from this Tube, as after it's Exit from a Hole in a very thin *Lamina*.

Therefore, to find out the Certainty of our Theory, let us make use of *Mariotte*'s Experiments concerning the Altitude of Fountains, in like manner as if the Holes that he made use of had been made in very thin *Laminæ*.

Let us therefore assume some one of his Experiments, which may be taken as a Foundation for finding the Altitude in the rest of the Experiments by our 12th Problem.

He indeed proposes that for a fundamental Experiment, where the Depth of Water in the Vessel is exactly 5 *Paris* Feet. But since ever so little an Error, suppose of 2 Lines, in this Experiment, may produce a considerable Error, namely of more than 8 Inches, in a 7 times greater Depth, which *Mariotte* uses afterwards; we will choose that Experiment for a fundamental one, in which that greatest Altitude, 7 times greater than the first, is applied.

Therefore let that Experiment of *Mariotte*, in which the Diameter of the Hole is 6 Lines and the Depth of Water in the Vessel 34 Feet, 11 ½ Inches, or 419 ½ Inches, *Paris* Measure, for the Foundation of our Inquiry.

When he applied this Altitude, he found the Water issuing from the Hole to rise to the Height of 31 Feet, 8 or 9 Inches, that is, to the Height of 380 ½ Inches.

* *Epist. ad Marinonium.*

Therefore

Therefore A = 419,5 Inch. a = 380,5 Inch. and α = 39 Inches.

In another Experiment, where E, or the Depth of Water in the Veſſel, is 26 Feet 1 Inch, the Water riſes thro' the ſame Hole, according to *Mariotte*, to the Height of 24 Feet, 2 ½ Inches. But e, or the Height of the ſalient Water, by *Prob.* XII. *Cor.* 1. is 24 Feet 3 Inches.

But, for the better comparing of the Altitudes, which *Mariotte* found the ſalient Water to reach, with thoſe Altitudes, to which it ought to ariſe by our Calculation, we have thrown both into Tab. I. where you ſee the Calculation to agree ſo with the Obſervations, that nothing can be better. And as theſe Experiments are made with the ſame Hole with the Diameter of 6 Lines, the Altitude only being changed, it can ſcarce be doubted, but our third Poſition, by which the *Reſiſtance*, *cæteris paribus*, is in a ſubduplicate *Ratio* of the Altitude, is right.

TAB. I.

Diameter of the Hole of 6 Lines.

Altitude of Water in the Veſſel.		Altitude of the ſalient Water, according to *Mariotte*.		Calculation.	
Feet	Inches	Feet	Inches	Feet	Inches
34.	11,5	31.	8,5	31.	8,5
26.	1	24.	2,5	24.	3
24.	5	22.	10	22.	10
12.	4	12.	0	11.	11
5.	6	5.	4,75	5.	5
5.		4.	11	4.	11,2 lin.
35.	5	32.	0	32.	1

TAB. II.

Diameter of the Hole of 4 Lines.

Altitude of Water in the Veſſel.		Altitude of the ſalient Water according to *Mariotte*.		Calculation.	
Feet	Inches	Feet	Inches	Feet	Inches
32.	11,5	30.	0	30.	0
24.	5	22.	8,5	21.	11
5.	6	5.	4,7	5.	4,4

TAB.

TAB. III.

Diameter of the Hole of 3 Lines.

Altitude of Water in the Veſſel.		Altitude of the ſalient Water according to *Mariotte*.		Calculation.	
Feet	Inches	Feet	Inches	Feet	Inches
34.	11,5	28.	0	28.	
26.	1	22.	0	22.	1
24.	5	22.	2	20.	11
5.	6	5.	4,7	5.	3,7

When inſtead of the Hole of 6 Lines *Mariotte* made uſe of the Hole of 4 Lines, he found the Water iſſuing from a Veſſel of the above-mentioned Altitude, 34 Feet 11 $\frac{1}{2}$ Inches, to reach the Height of 30 Feet. It ought to have reached by *Prob.* XII. *Cor.* 2. to 30 Feet 2 $\frac{1}{2}$ Inches nearly.

Afterwards when he uſed the Hole of 3 Lines, the Water iſſuing from the ſame Veſſel reached the Height of 28 Feet. It ought to have riſen by the ſame Corollary to 28 Feet 9 Inches nearly.

But theſe differences between the Altitudes from Calculation, and thoſe obſerved by *Mariotte* might proceed from a ſmall Error in taking the Diameters of ſuch ſmall Holes.

For if the *Radius* of the greateſt Hole, which *Mariotte* makes equal to 3 Lines, exceeded 3 Lines by $\frac{1}{50}$ Part of a *Paris* Inch; or if the *Radius* of the ſecond Hole, which *Mariotte* makes equal to 2 Lines wanted $\frac{1}{176}$ Part of a *Paris* Inch of 2 Lines; in either Caſe the Water will riſe by the Calculation to the Height of 30 Feet, as *Mariotte* obſerved.

Alſo if the *Radius* of the leaſt Hole was leſs than 1 $\frac{1}{2}$ Line by $\frac{1}{100}$ Part of a *Paris* Inch, and at the ſame Time the *Radius* of the greateſt Hole exceeded 3 Lines by $\frac{1}{100}$ Part of an Inch, the Calculation will give the Altitude of the ſalient Water 28 Feet, as *Mariotte* found it.

The Calculation being thus corrected *Tab.* II. and III. exhibit the Altitudes of *Mariotte* compared with our Calculation.

But here it muſt be obſerved, in *Tab.* II. that the Altitude of the Water ſpouting from a Veſſel of 24 Feet 5 Inches, according to *Mariotte*'s Obſervation, reaches to 22 Feet 8 $\frac{1}{2}$ Inches, and in *Tab.* III. that the Altitude of the Spout from the ſame Veſſel is 22 Feet 2 Inches, both which greatly exceed the Altitudes aſſigned by our Calculation.

But it is manifeſt, that *Mariotte*'s Numbers are corrupted. For,

1. The above-mentioned Rule of *Mariotte*, which, as he himſelf teſtifies, agrees well enough with the Obſervations, exhibits much ſmaller Numbers, which come pretty near to our Calculation.

2. It can never be, that the Water iſſuing from the Hole of 4 Lines ſhould reach the Height of 22 Feet 8 ½ Inches, nor that the Water iſſuing from the Hole of 3 Lines ſhould reach the Height of 22 Feet 2 Inches, for Water iſſuing from a Hole of 6 Lines reaches only to the Height of 22 Feet 10 Inches, which will eaſily appear from the Analogy of *Mariotte*'s Obſervations.

3. If the true Height is 22 Feet 2 Inches in *Tab.* III. the Water iſſuing from a Veſſel 24 Feet 5 Inches deep, riſes to a greater Height than when it iſſues from a Veſſel 26 Feet 1 Inch deep, which is manifeſtly abſurd.

Hence I am induced to believe, that *Mariotte*, when he ſpake of the firſt of theſe Experiments, wrote in his *Adverſaria*, *Le jet de quatre lignes n'a été plus bas que* d'onze pouces ou onze pouces & demi, *que celui dont l'ajutage étoit de ſix lignes*; whence *De la Hire* tranſcribed *plus bas que* d'un pouce ou un pouce & demi. Now this Correction being made, the Altitude obſerved by *Mariotte* will be 21 Feet 11 Inches, or 10 ½ which agrees exactly with our Calculation.

It will not ſeem ſtrange that ſuch Miſtakes ſhould happen, if we conſider, that *De la Hire* himſelf, who, after *Mariotte*'s Death, had the Care of printing his Papers, in the Preface to this Work ſpeaks in the following manner: *La moitié de cet ouvrage étoit aſſez au net pour étre imprimée; mais le reſte m'a donné beaucoup de peine à raſſembler ſur le memoires qui m'en ont été mis entre les mains apres ſa mort.*

But, every thing being well weighed, our Calculation agrees ſo well with the Experiments of this famous and diligent Obſerver, as alſo with *Poleni*'s Meaſure of effluent Water, and with the Meaſures of the Diameter of the contracted Vein taken by Sir *I. Newton* and *Poleni*, that it can hardly be doubted, but that the above Theory is either true, or very near the Truth.

It is eaſily extended to Water iſſuing thro' any ſquare or rectangular Hole, and alſo to an annular Hole, ſuch as ſurrounds Sir *I. Newton*'s *Circellus* *, whence many things deduced from the Contemplation of this *Circellus*, in the Reſiſtance of continuous Fluids muſt be altered; which ſeems neceſſary to be mentioned to the Learned, to excite them to a more accurate Examination of what has been ſaid.

An Account of a new Engine for raiſing Water, in which Horſes or other Animals draw without any Loſs of Power, (which has never yet been practiſed) and

II. The Animals all draw horizontally, and in a ſtrait Line, and at right Angles, whereby they exert their utmoſt Force. By theſe Advantages a far greater Power is gained from the Strength of Horſes, *&c.* than by their going round in a Circle; for by the Twiſt and Acuteneſs of the Angles, they draw in towards the Centre, whereby they waſte their Power, and alſo ſhorten their Levers: Beſides their Muſcles and Tendons from their hinder Legs all along their Sides to their Necks are unequally ſtrained, as the Duty is hardeſt on one Side, even tho' their Walk is large. Therefore each of thoſe Inconveniences muſt be attended

* *Princip.* Lib. II. Prop. xxxvi. Cor. 7, 8, 9, 10.

how the Strokes of the Pistons may be made of any Length, to prevent the Loss of Water, by the too frequent opening of Valves, with many other Advantages altogether new; the Model of which was shewn to the R. S. Nov. 28, by Walter Churchman, *the Inventor of it.* No. 434. p. 401. Sept. *&c.* 1734.

with Pain to the Animals when at Work, and a great Loss of their Strength.

2. A Crank does not rise quite ⅓ of it's Circle, neither do the Regulators or Rods rise or fall perpendicular, but obliquely, by which an oval Figure is made by the Piston's Motion in every Cylinder, which occasions great Friction and a Loss of Water, and every Arm of it is continually varying in it's Power whilst working, as it's Lever is distant from the perpendicular Line, and 2 of the Arms (supposing it a quadruple one) as they cross the Perpendicular are always drawing to and from their own Centre, by which the Power is not only lost, but the Time also; and farther yet, by the shortness of the Strokes, all the adjacent Water is frequently contrarily moved, and by the often opening and shutting of the Valves, there is also a great Waste of the Water, besides the many heavy Bearings, Frictions, Surges, and Repairs, belonging to it; all which Inconveniencies and Impediments being thoroughly considered, there must certainly be required a much greater Power to work the same than by my Method. For, hereby, a Stroke of 24 Feet will rise, and by enlarging or diminishing the fixed Wallower, you obtain a Stroke of any required Height, even to the extent of the Atmosphere's Pressure. By this great Advantage, the Water rises freer, and with greater Velocity, and as the Lifters or Forcers rise and fall exactly perpendicular, and with an equal continued Strain, and as the Bearings also are fewer and lighter, consequently the Friction in all these will be a great deal less than with the Crank, *&c.* And, Lastly, ⅞ of that Water which is always lost by the slow opening and shutting of the Valves will be saved.

From the above Considerations, and by the many Experiments I have made on this Occasion, in order to know the real Difference between these different ways of Working, I find, that near twice the Quantity of Water will be raised to the same Height, in the same Time, with the same Power, by my Method, more than with the best Crank-work that has ever been yet erected.

Description of the Engine. Fig. 102, 103, 104.

Fig. 102. *a. a. a. a.* Is the great Frame, the ends of which under the Pine-apples are to be contracted to the place of the little Frame, so that the Cross-piece at III. may support the 3 Bearings now shewn in the little one, for a better view only.

b. b. The little Frame on which the Cap Brasses are, which receive the turned T Gudgeons in the 3 horizontal Shafts.

c. c. The strong Supporters by the loose Wallowers.

d. d. The loose Wallowers, whose turned Rounds geer truly with the Coggs in the great Wheel.

e. e. e. The Regulator, which has a circular, direct, and retrograde Motion; see Fig, 103, 104.

f. f. The strong Shoulder or Stud fixed to the Shaft close by the Wallower, which stops this loose Wallower, when the End of the Regulator comes against it, thereby confining it for 2 Revolutions; after which it

quits

quits this Stud, and does the ſame on the oppoſite Side of the Wheel, and ſo on alternately to reverſe the Motion of the Stems in the different Cylinders.

g. g. The Wheels with their Coggs, which alternately work the fixed Wallower lying between them.

h. The fixed Wallower ſuppoſed to be of 4 Feet in Diameter (on a very ſhort Shaft) whoſe Rounds muſt be of caſt ſoft Iron, and truly turned, to elevate and depreſs the Racks to the Height of 24 Feet by it's 2 Revolutions.

i. i. i. i. The 4 Lifters or Forcers, behind each of which muſt be a ſmall Leverage back Wheel, truly fitted to direct the ſame to riſe and fall eaſily and exactly perpendicular, to avoid Friction and Loſs of Water in the Cylinders.

k. k. The large vertical Wheel, a ſmall Segment of which comes through the Floor in the Dome for the 4 Horſes to ſtand and Draw on.

l. m. The Arms, and the main Shaft of the ſame.

n. The turned T Gudgeon, with it's Collar and Shoulder, both of which muſt claſp the Rim of the under Leverage Wheel; to keep all firm and ſteady when in working.

o. The Leverage Wheel of about 4 Feet in Diameter, with a Braſs or Iron Rim ſuppoſed to be truly turned, and to have a ſtrong ſhort Iron Spindle through it's Centre, and at each End a turned Steel Collar and Shoulder bearing on 2 caſt Cap Braſſes exactly level, and ſunk into a ſtrong arched piece of Timber well braced and ſupported for this purpoſe.

p. p. Two ſmall ſide Leverage Wheels exactly fitted to the turned Part of the great Gudgeon, between the Collar and Shoulder: they are to be ſo placed and keyed, that their Friction from the Gudgeon may be alike when at Work.

q. q. The Steps which the Horſes Feet preſs, about 8 or 9 Inches broad, 2 Inches thick behind, and declining to an Edge, being deſigned to make level Ground and good footing for their hinder Legs when they draw.

r. r. Four Horſes only in view to avoid Confuſion, all drawing horizontally in a ſtrait Line, and at right Angles, whereby theſe uſeful Animals will ſoon be taught a new and pleaſant way of working to themſelves, a more advantageous one to their Maſters, and of greater Utility to the Publick.

s. The faſtening places behind the Horſes, ſuppoſed to be ſtrong Arms below in the Supporter, and a Croſs-Bar above, at both of which you may place ſmall Sheeves or Rollers; the upper Part of them to be level with each Horſe's Breaſt (when drawing) and the Rope or Strap to come over the ſame, in order to keep a Weight ſuſpended of 300 ℔ more or leſs one or two Inches from a Plank. By this Method you will be exactly informed of the Strength of each Horſe, how long it continues,

and when to relieve him, as alſo when juſtly to correct the ſlothful one, whoſe Weight reſting on the Plank will always diſcover his Lazineſs.

t. The faſtening Places before, being deſigned to direct their Heads.

u. The Dome merely for Ornament; in the place of which, erect a Workloft, over that a horizontal Windmil; on the lower End of it's upright Shaft, fix a Spur-Wheel to work with the Coggs of the great Wheel, thereby to aſſiſt the Horſes, or when there is a ſufficient Force of Wind to do their whole Duty.

w. The Coupling Staples with their Braſſes.

x. The Strong Catch which confines the great Wheel to the Frame.

y. The Screw or Key-band to confine all cloſe and tight.

z. The Cylinders which are ſcrewed together at their Ends out of Sight.

&. All the ſame ſort of Work chiefly for Uniformity in the Draught.

N. B. A ſingle Shaft with the looſe and fixed Wallowers, will be of great Simplicity and Advantage to the Publick, as being erected for leſs Expence, and as it will work pleaſantly any Number of Racks for lifting or forcing, at either of it's Ends, or at both together: But chiefly, as it is eaſily adapted to the different ſorts of Windmils, Waterwheels, &c. of all Denominations already in Uſe. It alſo ſerves for ſmall Purpoſes, *Vid.* Fig. 103. The Pins 4, 4, and the Arms 5, 5, which claſp the Braſſes 6, 6, with the oval Figure 7 and it's 2 Teeth, make this Regulator, which is worked by the Stud in the main Shaft.

☞ In large Engines and Machines where the Motion is regular, every heavy Bearing ſhould have one of theſe Wheels, for they ſave Power by greatly abating Friction. Upon the Principle of theſe Leverage Wheels, Capt. *Rowe* has publiſhed what he calls his Friction-Wheels, tho' Subſequent to my Specification thereof.

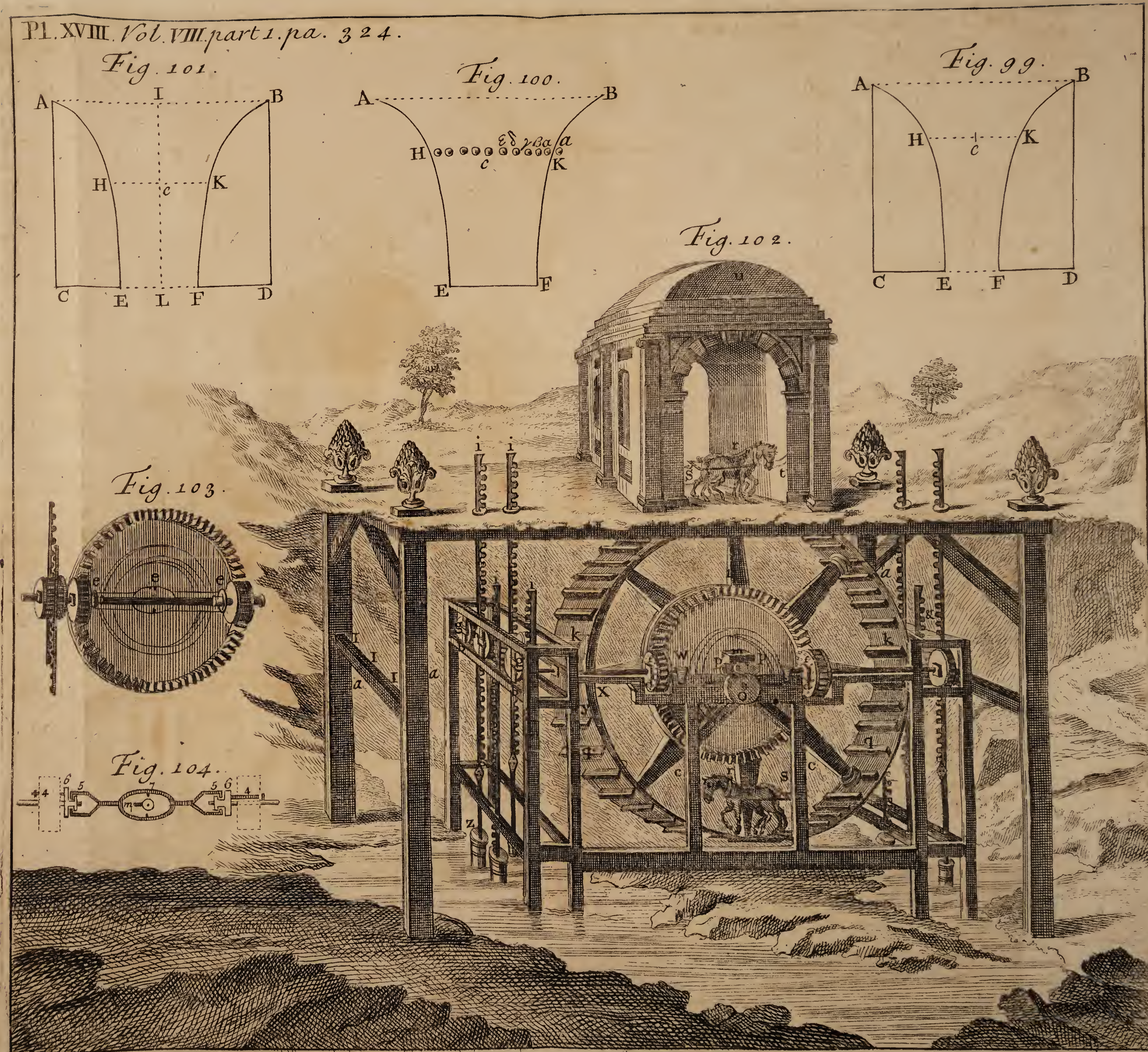
CHAP. VII.

GEOGRAPHY and NAVIGATION.

Of the Figure of the Earth, and the Variation of Gravity on the Surface. By Mr James Stirling, *F. R. S.* No. 438. p. 98. July, *&c.* 1735.

I. THE Centrifugal Force, ariſing from the Diurnal Rotation of the Earth, depreſſeth it at the Poles, and renders it protuberant at the Equator; as has been lately advanced by Sir *I. Newton*, and long ago by *Polybius*, according to *Strabo* in the 2d Book of his *Geography*. But although it be of an oblate ſpheroidical Shape, yet the kind of that Spheroid is not yet diſcovered; and therefore I ſhall ſuppoſe it to be the common Spheroid generated by the Rotation of an Ellipſis about it's leſſer Axis; although I find by Computation, that it is only nearly, and not accurately ſuch. I ſhall alſo ſuppoſe the Denſity to be every

 where


Pl. XVIII. Vol. VIII. part 1. pa. 324.
Fig. 101.
Fig. 100.
Fig. 99.
Fig. 102.
Fig. 103.
Fig. 104.

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where the ſame, from the Center to the Surface, and the mutual Gravitation of the Particles towards one another, to decreaſe in the duplicate Ratio of their Diſtances: And then the following Rules will follow from the nature of the Spheroid.

1. Let A D B E be the Meridian of an oblate Spheroid, D E the Axis, A B the Diameter of the Equator, and C the Center. Take any Point on the Surface, as F, from which draw F C to the Center, F G, perpendicular to the Surface at F, meeting C B in G, and F H cutting the Line C G, ſo that C H may be to G H as 3 to 2. I ſay that a Body at F will gravitate in the Direction F H; and that the mean Force of Gravity on the Surface will be to the Exceſs of the Gravity at the Pole above that at F, as the mean Diameter multiplied into the Square of the Radius is to $\frac{1}{5}$ of the Difference of the longeſt and ſhorteſt Diameters multiplied into the Square of the Coſine of Latitude at F. Fig. 105.

2. The Decrement of Gravity from the Pole to the Equator is proportional to the Square of the Coſine of Latitude; or, which comes to the ſame, the Increment of Gravity from the Equator to the Pole is proportional to the Sine of Latitude. Hitherto I have conſidered the Variation of Gravity which ariſes from the ſpheroidical Figure, while it does not turn round it's Axis; but if it doth, the Direction of Gravity will be in the Line F G, perpendicular to the Surface; and it's Variation now ariſing from both the Figure and centrifugal Force, will be 5 times greater than what ariſes from the Figure alone; as will appear from the Proportion of the Lines F H and F G, the former being to the latter, as the whole Force of Gravity at F, while the Spheroid is at Reſt, to the Force with which a Body deſcends at F, while it turns round it's Axis.

3. From this laſt Article it appears, that $\frac{1}{5}$ of the Variation of Gravity is occaſioned by the Figure of the Spheroid, and the remaining $\frac{4}{5}$ by the centrifugal Force. And whereas the Earth could not be of an oblate ſpheroidical Figure, unleſs it turned round it's Axis; nor could it turn round it's Axis, without putting on that Figure: I ſay, that the Diminution of Gravity towards the Equator, known by the Experiments with Pendulums, prove both the Rotation and oblate ſpheroidical Figure of the Earth.

4. The mean Force of Gravity on the Surface is to the centrifugal Force at any Point F, as a Rectangle under the Radius and mean Diameter to a Rectangle under the Coſine of Latitude, and $\frac{4}{5}$ of the Difference of the longeſt and ſhorteſt Diameters. And at the Equator, where the Coſine of Latitude becomes equal to the Radius, the mean Force of Gravity is to the centrifugal Force, as the mean Diameter to $\frac{4}{5}$ of the longeſt and ſhorteſt Diameters. This Article is found from the Proportion of the Lines F H and G H; the former being to the latter as the Force of Gravity to the centrifugal Force.

5. The Proportion of the Diameters of the Earth will be found in the following manner: The Moon revolves about the Earth in 27^d, 7^h, $43'$,

or

or in 39343 Minutes: And her mean Diſtance is about 59 ½ Semidiameters of the Earth, according to *La Hire*'s and *Flamſtead*'s Tables; but near 60 ½ by *Halley*'s Tables. I ſhall therefore take 60 for the mean Diſtance, till it be better known: Then according to the Nature of Gravity, as the Cube of the Moon's Diſtance to the Semidiameter of the Earth, or as 216000 to Unity, ſo is 1547870000 the Square of the periodick Time of the Moon to 7166, the Square of the Number of Minutes in which another Moon would revolve about the Earth at the Diſtance of it's Semidiameter. And as this laſt Number to 2062096, the Square of 1436, the Number of Minutes in a Sydereal Day, ſo is Unity to 287.7; which would ſhew the Proportion of the centrifugal Force at the Equator to the mean Force of Gravity (by *Corol.* 2. *Prop.* 4. *Lib.* 1. *Princip.*) were it not for the Action of the Sun on the Moon. Therefore (by *Corol.* 17. *Prop.* 66. *Lib.* 1. *Princip.*) I ſay, As the Square of the Sydereal Year, to the Square of the periodick Time of the Moon, that is, as 179 to Unity, So is 287.7 to 1.6; which being added to 287.7, makes 289.3. And therefore, As Unity to 289, neglecting the Fraction which is uncertain, So is the centrifugal Force at the Equator to the mean Force of Gravity on the Surface. And thence (by Article 4.) As 289 to ¼, So is the mean Diameter to the Difference of the longeſt and ſhorteſt: And therefore, As the Axis is to the equatoreal Diameter, So is 2307 to 2317, or in ſmaller Numbers, As 231 to 232, the ſame as Sir *I. Newton* found in a different manner, for he makes it as 230 to 231, and as 230 to 231, So is 231 to 232.004.

6. In the ſame manner the Proportion of the Diameters of any Planet may be found, if it has a Satellite: For Inſtance, in *Jupiter*, he turns about his Axis in $9^h. 56'$, or in 596 Minutes, and his third Satellite revolves about him in $7^d, 3^h, 42', 36''$, or in 10302.6 Minutes, at the diſtance of 15.141 of his Semidiameters. Therefore, I ſay, As the Cube of 15.141 to Unity, So is the Square of 10302.6 to 30579, the Square of the Number of Minutes in which a Satellite would revolve about him at the diſtance of his Semidiameter: And as this laſt Number is to 355216, the Square of 596, ſo is Unity to 11 ⅝, or the centrifugal Force at his Equator to the mean Force of Gravity on his Surface. There is no need of correcting this Number, as in the former Article, becauſe the periodick Time of *Jupiter* round the Sun is vaſtly greater than that of his third Satellite round him. I have choſen the third Satellite before any of the reſt, becauſe it's greateſt Elongation was obſerved by Dr *Pound*, with a Micrometer adapted to a Teleſcope 123 Feet long; and he alſo took the Diameter of *Jupiter* by the Tranſit of the Satellite, which is a much more exact Way than with a Micrometer. But as the Planes of *Jupiter*'s Satellites almoſt coincide with the Plane of his Equator, the Diameter, determined by the Tranſit of the Satellite, is his greateſt; and the Diſtance of the Satellite, which ought to have been given in his mean Diameters, is aſſigned in his greateſt: For which Reaſon the Force of Gravity already found, muſt be augmented in the triplicate

triplicate Ratio of his greateſt Diameter to his mean one; that is, if a repreſents the mean Diameter, and d the Difference of the longeſt and ſhorteſt, in the Proportion of $2a + 3d$ to $2a$ very nearly. Hence, as the centrifugal Force at his Equator, to the mean Force of Gravity on his Surface, ſo is Unity to $11\frac{5}{8} \times \frac{2a+3d}{2a}$. And (by Article 4.)

$$11\tfrac{5}{8} \times \frac{2a+3d}{2a} : 1 :: a : \tfrac{4}{5}d, \text{ or } 20aa = 186ad + 279dd;$$

which makes a to d, as 108 to 10; and thence the Axis is to the equatoreal Diameter, as $108 - 5$ to $108 + 5$, or as 103 to 113; that is, as 12 to $13\frac{1}{6}$: Which agrees nicely with the Obſervations of both Dr *Pound* and Mr *Bradley*, made with *Huygens*'s Long Teleſcope; the former making it as 12 to 13, and the latter as 25 to 27, which is very nearly the ſame. And if this Theory agrees ſo well with Obſervations in *Jupiter*, there is no doubt but it will be more exact in the Earth, whoſe Diameters are much nearer to Equality.

7. By Experiments made at *Jamaica** in the Latitude of 18° with a very curious Clock, contrived by Mr *Graham*, it was found that the *London* Pendulum went ſlower there by 2′ 6″ in a Sydereal Day, than at *London*. But it was found by Experiments made with Thermometers, that 9″ were to be allowed for the lengthening of the Pendulum by Heat; and therefore it was retarded only 1′ 57″ by the Decrement of Gravity. So that while a Pendulum of *London* makes 86164 Vibrations, the Number of Seconds in a Sydereal Day, the ſame at *Jamaica* only gives 86047 Vibrations. Therefore the Force of Gravity at *London* is to that in the Latitude of 18°, as the Square of 86164 to the Square of 86047; that is, very nearly as 1106 to 1103. And (by Article 1, and 2.) if a denote the mean Diameter of the Earth, d the Difference of the greateſt and ſmalleſt;

$a - \frac{ccd}{rr}$ will denote the Force of Gravity in general in any Latitude, whoſe Coſine is to the Radius as c to r: Where, if in the Place of c there be ſubſtituted the Coſines of 51° : 32′ and 18° : 0′, that is of the Latitudes of *London* and *Jamaica*, we ſhall have the Force of Gravity at the former to that at the latter, as $a - .3870\,d$ to $a - .9045\,d$, that is as 1106 to 1103. Whence the mean Diameter of the Earth will be to the Difference of the Axis and equatoreal Diameter, as 191 to Unity; and thence (by Article 4.) as the mean Gravity on the Surface to the centrifugal Force at the Equator, ſo is 191 to $\frac{4}{5}$, or ſo is 239 to Unity. In order to ſhew that this cannot be, I ſhall obſerve, that when the Moon's Diſtance was ſuppoſed 60 Semidiameters of the Earth (as in Article 5.) it was found that the mean Force of Gravity was to the centrifugal Force at the Equator, as 289 to 1. But if the Proportion now found be true, the Moon's Diſtance of 60 Semidiameters muſt be augmented in the ſubtriplicate Proportion of 289 to 239, and then it will

* See Chap. V. §. III.

become

become 64 Semidiameters. In the like manner, if we compute the Ratio of the mean Force of Gravity to the centrifugal Force, by presupposing the Magnitude of the Earth, as Sir *I. Newton* and Mr *Huygens* did, we must suppose a Degree to be above 80 *English* Miles to bring it out 239 to Unity. Now whereas it is certain that the Distance of the Moon is about 60 Semidiameters of the Earth, and that a Degree is less than 70 *English* Miles; therefore, I say, that the Conclusion which seems to follow from the *Jamaica* Experiment, cannot be allowed to be true. And the Experiments made by *Richer*, in the Island of *Cayenna*, would still make a greater Difference betwixt the Diameters of the Earth, than those made in *Jamaica*. And the Lengths of the *Paris* and *London* Pendulums compared together, would make it greater than one 231 Part of the Whole, as it was found in Article 5.

8. From all the Experiments made with Pendulums, it appears that the Theory makes them longer in Islands, than they are found in Fact. The *London* Pendulum should be longer when compared to the *Paris* one, than it really is: The *Jamaica* Pendulum, when compared to the *London* one, which vibrates in a greater Island, should be longer than is found by Experience; and the Pendulum in *Cayenna* (a smaller Island than *Jamaica*) should still be longer. This Defect of Gravity in Islands is very probably occasioned by the Vicinity of a great Quantity of Water, which being specifically lighter than Land, attracts less in Proportion to it's Bulk. And I find by Computation, that the Odds in the Pendulums betwixt Theory and Practice is not greater than what may be accounted for on that Supposition. I shall also observe, that although the Matter of the Earth were entirely uniform, yet the Hypothesis of it's being a true Spheroid is not near enough the Truth to give the Number of Vibrations which a Pendulum makes in 24 Hours. And suppose the true Figure were known, the Inequalities of Mountains and Vallies, Land and Water, Heat and Cold, would never allow Theory and Experiments to agree. But after the *French* Gentlemen who are now about measuring a Degree, and making Experiments with Pendulums in the North and South, shall have finished their Design, we may expect new Light in this Matter.

Some Investigations, by which it is proved, that the Figure of the Earth must very nearly approach to an Ellipsis, according to the Laws of Attraction, in an inverse Ratio of the Square of the Distances,

II. According to Sir *I. Newton*'s *Principia* (*Cor.* 3. *Prop.* XCI. *Lib.* 1. and *Prop.* XIX. *Lib.* 3.) if an elliptic Spheroid, consisting of fluid and homogenous Particles mutually attracting each other, in an inverse *Ratio* of the Square of the Distances be revolved round it's *Axis* A *a*, that the Columns C E, C N, C A, of which that Spheroid is composed, may be placed *in Æquilibrio*, and so the Spheroid may always have the same Figure, the Gravity in any Point of the Surface N must necessarily be in an inverse *Ratio* of the *Radius* C N.

That we may know therefore, whether the Spheroid has this Property, let us now seek what Attraction is suffered by every Corpuscle N, of the whole Spheroid according to the Direction C N; and from that Attraction let us take that Part of the centrifugal Force, which proceeds from

from the Rotation of the Spheroid acting according to C N, and let us seek whether the remaining Force is proportional to $\frac{1}{CN}$. Therefore we will first investigate the following; and as our Intention is to apply our Discoveries to the Spheroid of the Earth, which all agree to be very little different from a Sphere, our Computations must be adapted to those Spheroids, the greater *Axis* of which exceeds the lesser by the very smallest Quantity. *by M.* Alexis Clairaut, *F.R.S. and R. Acad. Sc. Paris.* No. 445. p. 19. Jan. *&c.* 1737.

Prob. I. *To find the Attraction, which the Spheroid* A E *a e*, differing very little from a Sphere, exercises on a Corpuscle situated at the Pole A. Fig. 106.

For the Solution of this Problem we should repeat; *Cor.* 2. *Prop.* 91. *Newt. Princip.* by which you may learn the manner of finding the Attraction of any Spheroid, if you substitute in the general Value for C E the Quantity which differs infinitely little from A C; but as in that case the Problem comes out much easier, we shall solve it after the following manner.

Let A M D *a d* be a Sphere, of which the *Radius* is A C: We will seek the Attraction of the Space which rises from the Revolution A D *a* E, which Attraction, being added to the Attraction of the Sphere, gives the Attraction sought.

To find the Attraction of the Space arising from the Revolution A N E *a* D M, let A C be r, D E, αr, A P, u, then from the Nature of the Ellipse N M $= \alpha\sqrt{2ru - uu}$; but from the Nature of the Circle A M $= \sqrt{2ru}$. But the Space arising from the Revolution

N *n m* M will be $\frac{\alpha c}{r}\ \overline{2ru - uu}\,.\,du$, for c is the Circumference, and r the *Radius*.

Because of the Smallness of N M, we may account all the Particles of Matter contained in that Space as equally attracting the Corpuscle in A: wherefore you will make but little account of the Attraction of that Space, if you multiply it's Solidity by the Attraction in M. But that Attraction in M ought to be $\frac{1}{AM^2} \times \frac{AP}{AM}$. You will therefore have analytically $\frac{u}{2ru\sqrt{2ru}} . \frac{\alpha c}{r} . \overline{2ru - uu} . du = \frac{\alpha c}{2rr\sqrt{2r}}$ $(2r\,du\sqrt{u} - u\,du\sqrt{u})$ of which the Integral $\frac{\alpha c}{2rr\sqrt{2r}}$ $\left(\frac{4}{3}ru\sqrt{u} - \frac{2}{5}uu\sqrt{u}\right)$ is the Attraction of the Space arising from the Revolution A N M. In which Value, if you make $u = 2r$, you will

will have by Reduction $\frac{8}{15} c \alpha$; whence the Attraction of the whole Space A E a C is expressed, and by adding afterwards $\frac{2}{3} c$ for the Attraction of the whole Sphere, you will have $\frac{2}{3} c + \frac{8}{15} c \alpha$, the Attraction of the Ellipsoid.

Coroll. If you would have an oblong Spheroid, α will be negative, but the Sum of the Attraction will be $\frac{2}{3} c - \frac{8}{15} c \alpha$.

Note. If the above Spheroid, instead of circular Elements arising in P N, consisted of other Elements, for Instance, Elliptical, which should differ from a Circle no more than the Ellipsis A E, and should have the same Surface as the Circles P N, the Attraction would manifestly be always the same, because in those Elements P N, whatsoever the remaining Force should be, the Circles P M being taken away, it would be as it were composed of Parts which would have the same Attraction as upon that of the Ellipsoid, having regard to the Smallness of N M, and the Quantity of equable Matter.

Lemma. Fig. 107. Let K L be a Circle, H the Centre of the Circle, V H a Perpendicular in the Area of the Circle, and N H a Line equal to the Perpendicular V H, which shall make therewith an Angle infinitely small or very small, I say that the Attraction of the Circle K L in N, may be taken without any sensible Error as the Attraction of the Circle in V, or, which is the same thing, that one Attraction does not differ from the other but by a Quantity infinitely less with respect to both, than V N is less in respect to H V.

To demonstrate which Proposition, it must be shewn, that, 2 Corpuscles being placed at the extremity of any Diameter K L, there is one attractive Force in N, and another Force in V, of which the Sum may be reckoned the same. But neglecting the Computation to have the Attraction of a Body placed in K to the Corpuscle N, you may easily see, that it will be the same with the Attraction in V, to which a small Quantity should be added, which N V should enter. In like manner also you may see, that the Attraction of the Body placed in L to the Corpuscle N will be the same with the Attraction in V, taking away the same small Quantity. Therefore the Sum of both these Attractions is one and the same.

Coroll. If instead of the Circle K L there was a certain *Ellipsis*, or any other curve Line, which should differ very little from a Circle, by the same Arguments, which were used in the *Note*, it is easily gathered that there would always be place for the foregoing Proposition.

Let

Let A E a e be an Elliptic Spheroid, of which let A a be the *Axis* of Revolution. I ſay that the Attraction, which this Spheroid exerciſes to the Corpuſcle placed in N, is the ſame with that Attraction, which every Spheroid exerciſes, whoſe Pole ſhould be N, Axis of Revolution N n, and ſecond *Axis* the *Radius* of a Circle, which ſhould have the ſame *Superficies* as the *Ellipſis* F G, a Section of the *Ellipſoid* A E a e thro' a Plane erected perpendicularly on F G, it's conjugate Diameter. Theorem I. Fig. 108.

To Demonſtrate this, imagine innumerable Elements K L, parallel to the *Ellipſis* F G, that is, all erected upon Ordinates to the Diameter. It is evident, that the Spheroid A E a e will differ from the aforeſaid Spheroid only in this, that in the firſt all the Elements make an Angle with C N differing from a right Angle by an Angle infinitely ſmall, but in the ſecond all the Elements make a right Angle without any Difference, whereas in both Spheroids the Elements have the ſame *Superficies*. But, by the preceding Propoſition, the Attraction of every Element K L to N is thought in a manner the ſame in both Caſes; but as for the Thickneſs of the Elements, K k l L, we may take H h for the Perpendicular h i, becauſe of the Smallneſs of the Angle i h H; therefore the total Attraction of both Spheroids may be taken one in the Room of the other.

To find the Attraction of the Spheroid A E a e, *to a Corpuſcle placed in any Point* N. Prob. II.

Let A C $= a$, C E $= b$, C N $= r$, C G the conjugate Diameter, C N will be $\frac{ab}{r}$ (ſince a and b differ very little between themſelves) we muſt (by the preceding Propoſition) ſeek the Attraction of the Spheroid, whoſe greater *Axis* is r, and leſſer $\sqrt{\frac{abb}{r}}$, or $b\sqrt{\frac{a}{r}}$.

To this we muſt apply the *Formula* which we found in *Prob.* I. $\frac{2}{3}c - \frac{8}{15}c\alpha$, or $\frac{2}{3}pr - \frac{8}{15}pr\alpha$ (putting pr for c) but inſtead of α in this Formula, we muſt ſubſtitute $\frac{r - b\sqrt{\frac{a}{r}}}{r} = 1 - \frac{b}{r}\sqrt{\frac{a}{r}}$, or $\frac{3}{2}n - m$, if you put $a + ma$ for b $a + na$ for r, and in the Computation neglect the ſecond Degrees of the Magnitudes n and m.

 If

If therefore you put $\frac{3}{2}n - m$ in the place of α, the aforesaid *Formula* will become $\frac{2}{3}pr - \frac{4}{5}prn + \frac{8}{15}prm$, or $\frac{2}{3}pa - \frac{2}{15}pan + \frac{8}{15}pam$; which is the Expression of the sought Attraction of the Spheroid in N.

If $n = o$, then you may have $\frac{2}{3}pa + \frac{8}{15}pam$ for the Attraction in a, that is, to the Pole.

But if $n = m$, then you may have $\frac{2}{3}pa + \frac{6}{15}pam$ for the Attraction to the Equator.

Theorem II. Fig. 106. Let A E a e be a Spheroid as above, whose *Axis* differs by a very small Quantity, which, for the greater Perspicuity, I shall call infinitely small. If this Spheroid is conceived to be of a fluid and homogenous Matter, and turned about the *Axis* A a, in a congruent Time, that the Gravity of the Column C E may be equal to the Gravity of the Column A C, that is, by Sir *I. Newton*'s Principles, the Attraction in E, the Centrifugal Force being taken away, may be to the Attraction in A, as C A to C E: I say, that all the Columns C N, wanting an infinitely small of the second Order, will preserve an *Æqilibrium* with those 2 Columns; that is, the Attraction in N, taking away the centrifugal Force made simple according to C N, is to the Attraction in A as C A to C N.

Let the same Denominations be preserved for the Demonstration, which were used in the preceding Proposition; first let the centrifugal Force in E be sought, which may agree with the *Æquilibrium* of the Columns C E, C A.

Therefore say $\frac{2}{3}pa + \frac{6}{15}pam - f : \frac{2}{3}pa + \frac{8}{15}pam :: 1 : 1 + m$, whence is drawn $f = \frac{8}{15}pam$.

Then to apply the Gravity in N compounded of the Attraction, taking away the centrifugal Force, the centrifugal Force in N is to be sought, or, which is the same thing, in M above the Sphere, because they ought to differ from each other only by an infinitely small of the second Order, if D E is supposed to express the centrifugal Force f in E, M N will express the centrifugal Force in N, but the centrifugal Forces are as *Radii*, when the Times of Revolutions are the same, but by the property of the *Ellipsis* it becomes as D E : N M :: C E : M P.

But if the centrifugal Force acts according to N P, it must be reduced according to N C, and N O will be the remaining Part. Therefore the

the centrifugal Force in N or in M is to the centrifugal Force in E or in D, as N O is to D E. Therefore the Expreſſion of the centrifugal Force in N will be $\frac{8}{15}\,p\,a\,n$, and conſequently the Expreſſion of the Gravity will be $\frac{2}{3}\,p\,a - \frac{2}{15}\,p\,a\,n + \frac{8}{15}\,p\,a\,m - \frac{8}{15}\,p\,n\,a$, or $\frac{2}{3}\,p\,a - \frac{2}{3}\,p\,n\,a + \frac{8}{15}\,p\,a\,m$.

Now to find the centrifugal Force in N, which follows from the *Æquilibrium* of the Columns, the Gravity in A muſt be to the Gravity in N, as N C to A C, the Gravity in A is $\frac{2}{3}\,p\,a + \frac{8}{15}\,p\,a\,m$, which Expreſſion being drawn into $\frac{1}{1+n}$ or $1 - n$, after Reduction will become $\frac{2}{3}\,p\,a - \frac{2}{3}\,p\,n + \frac{8}{15}\,p\,a\,m$, and is the ſame Expreſſion with that above. Thence we may ſee, that there can be but an infinitely ſmall Difference between the Figure which the Earth ought to have by the Newtonian *Hypotheſis*, and the Ellipſoid. For as the Quantity D E is about $\frac{1}{230}$ Part of A C, in the preceding Computation, we neglect only the Quantities of the ſame Order with $\frac{1}{230 \cdot 230^2}$.

III. 1. That the Figure of the Earth is Spheroidical is agreed upon by all: But whether it be an oblong or oblate Spheroid, *i. e.* whether the Axis be longer or ſhorter than a Diameter at the Equator, has been for ſome time a matter of Doubt. Three ſeveral Methods have been propoſed to determine this Controverſy by Experiments; as by the different Lengths of Pendulums vibrating Seconds, in different Latitudes; the Figure of the Earth's Shadow in Lunar Eclipſes; and by the actual Meaſurement of the Lengths of a Degree on the Meridian in different Latitudes.

An Account by John Eames, *F. R. S. of a Diſſertation, containing Remarks upon the Obſervations made in* France, *in order to aſcertain the Figure of the Earth, by* Mr Celſius, *intituled,* De Obſervationibus pro Figura Telluris determinanda, in *Gallia* habitis, Diſquiſitio. Auctore *Andrea Celſio,* in

It is certain, if the Lengths of the Degrees of Latitude decreaſe as we go from the Equator toward the Poles, then the Axis is greater, and the Figure an oblong Spheroid; but, on the contrary, if theſe Lengths increaſe as you remove towards the Poles, the Axis is leſs than a Diameter at the Equator, and conſequently an oblate Spheroid.

M. *Caſſini* and others, judge the Earth to be of an oblong Spheroidical Figure; and the Obſervations made in *France*, if entirely to be depended upon, prove this *Hypotheſis* to be a Matter of Fact. Our late illuſtrious Preſident, Sir *Iſaac Newton*, Mr. *Huygens*, and others, make the Earth

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Acad. *Upsal.* Astronom. Prof. Reg. &c. *Upsaliæ*, 1738. 4to. No. 457. p. 371. *July*, &c. 1740.

to be an oblate Spheroid, higher at the Equator than at the Poles; and this Figure of the Earth is undoubtedly the true one, if the Observations lately made near the Arctic Circle be admitted as certain and exact. So that since both Sets of Observations have been taken by Persons of known Skill, Dexterity, and Integrity, it is now become absolutely necessary to inquire into this Matter, in order to find out the Occasion of so great a Difference in their Conclusions.

Mr *Celsius*, in the Treatise before us, proposes to consider this Matter more closely, and begins with a Defence of the Observations made at *Tornea*, near the North Polar Circle; and then takes Notice of some Things, proper to be considered, relating to the Instruments, Astronomical Observations, and Trigonometrical Operations, performed in *France*; which, in his Judgment, render the Observations uncertain; at least so far as not to be accurate enough to be depended upon in determining the Matter in Question.

To begin with the Defence of the Observations made at *Tornea*: Perhaps it may not be improper to premise a short Account of them. They were undertaken at the Charge of the King of *France*, by 5 skilful Gentlemen; 3 of them Members of the *Royal Academy* at *Paris*, who were joined by Mr *Celsius*, and the Abbé *Authier*. The Trigonometrical Part of the Work was performed near the River of *Tornea*, whose Direction is the same with the Meridian of *Tornea*; the Coasts of the Gulph of *Bothnia* being found very inconvenient for that Purpose. By the favourable Situation of 5 Mountains they formed 8 Triangles, which took in Space enough for their Design. All the 5 Gentlemen observed, one after another, each Angle of these Triangles, setting them down in writing separately.

They afterwards determined the Distance between *Tornea* and Mount *Kittis*, under the same Meridian, by a Basis, measured on the River when frozen over, whose Length was 7406 Toises 5 Feet, by the first Measurement; and when measured again, was barely 4 Inches over. This Distance between them they found to be 55,234 Toises.

The first Part of their Work being thus finished, the next was to find the Difference of Latitude of these two Places: This they did by the Help of a Telescope, fixed to a Sector of 9 Foot, made at *London*, by the Care and Direction of Mr *George Graham*. The Star they observed at *Tornea* was *α Draconis*: They repeated their Observations 3 Times, and the greatest Difference between them was but 2″: Removing to Mount *Kittis*, they took the same Number of Observations, of the same Star, without finding more than 1″ Difference. The Result was, that the Amplitude of the Arch, in the Heavens, between *Tornea* and Mount *Kittis*, (allowing for the Precession of the Equinox, and the Time elapsed between the 2 Observations, according to Mr *Bradley*'s Theory) was 57′ 26″. Hence the Magnitude of a Degree, on the Earth, intersecting the Polar Circle, was found to be greater than a mean Degree of *France* 377 Toises; and to differ 900 Toises

from

from what it ſhould have been, according to M. *Caſſini*'s Hypotheſis: And if the Correction, according to Mr *Bradley*'s Theory, were omitted, the Difference would have amounted to above 1000 Toiſes: The Conſequence of which, ſay the curious Obſervers, is, That the Earth is not only flatted towards the Poles, but that it is much more ſo than Sir *I. Newton* or M. *Huygens* thought it. This unexpected Difference being ſo very great, made them reſolve upon a careful as well as new kind of Verification of the Whole. In the firſt Place, they repeated their Aſtronomical Obſervations 3 ſeveral Times, at *Tornea* and *Kittis*, with the ſame Inſtrument, but on another Star, *viz.* δ *Draconis*: The Difference of Latitude between the 2 Places was found to be the ſame, within $3\frac{1}{2}''$, with the Firſt. They then not only examined the Truth of their Meridian Line, the Exactneſs of the Sector, in the different Diviſions upon the Limb, chiefly in the 2° imployed in obſerving α & δ *Draconis*, but ſuppoſed that, in their Trigonometrical Operations, they had erred in each Triangle, by $20''$ in each of the 2 Angles, and $40''$ in the Third; and that all theſe Errors tended to diminiſh the Length of the Arch; the Calculation, upon this Suppoſition, gives but $44\frac{1}{10}$ Toiſes for the greateſt Error that could be committed.

When a particular Relation of all theſe Obſervations was read before the *Royal Academy of Sciences* at *Paris*, and inquired into; the main Exception taken to them was, That the Obſervers, omitting to make a Proof of the Line of Collination, by Means of double Obſervations, with the Face of their Inſtrument turned contrary Ways, have thereby not duly aſcertained the Truth of their Obſervations. But this Objection was fully anſwered by M. *Maupertuis*, as Mr *Celſius* hopes and believes, to the entire Satisfaction of M. *Caſſini*, who made it. He allows M. *Caſſini* had very good Reaſon to mention this, as a Thing proper to be done in Inſtruments of common Uſe for this Purpoſe, which generally ſtand in Need of ſuch a Method of Verification: But it was not at all neceſſary in the Inſtrument uſed at *Tornea* and Mount *Kittis*: The very Make of it was ſuch, that no Alteration could eaſily be made in it, ſo as to create any perceptible Error in the Obſervations. The whole Apparatus of the Teleſcope and Sector is all framed together; the Object-glaſs and Croſs-wires, as well as the Limb, ſo firmly fixed to the Tube, as not to be diſlocated without great Violence. Notwithſtanding all this, the utmoſt Care was taken in tranſporting it from one Place to another; being placed in a Cheſt, that the *Laplanders*, to uſe his own Words, *in illa ciſta idolum quoddam ſervari facile ſibi perſuaderent.* He adds, the ſame Objection may be made to M. *Picard*'s Obſervations, who does not ſeem to have uſed this Precaution, as M. *Caſſini* himſelf acknowledges, who neverthele ſs approves and extols his Obſervations for their Accuracy: So that thoſe at the Arctic Circle may be very good, notwithſtanding the Want of this, ſuppoſed neceſſary, Operation. And indeed, that they were ſo, ſufficiently appears from this Fact: The Difference of Latitude between *Tornea* and Mount *Kittis*, found in

Sep-

September, was obſerved again in *March* following, by the Help of the ſame Star δ *Draconis*, and did not differ from the former above $3\frac{1}{2}''$, though the Inſtrument had been twice carried from one Place to the other. This is a Degree of Exactneſs not eaſy to be met with; no not in M. *Caſſini*'s Obſervations, made on different Stars, which differ ſometimes $40''$, in determining the Amplitude of an Arc in the Heavens, though their Inſtrument was carefully examined in the Way above-mentioned.

The Author then proceeds, in his Turn, to inquire into the Accuracy and Certainty of the two Sets of Obſervations made in the North and South Parts of *France*, in reſpect of the Royal Obſervatory at *Paris*.

As to the Meaſures of the Degrees in the Northern Parts of *France*, between *Paris* and *Dunkirk*, he owns they cannot be much out of the Way; being in ſome Meaſure confirmed by M. *De la Hire*, in the Year 1683, and M. *Caſſini* himſelf. Yet Mr *Celſius* obſerves, that the Baſis on the ſandy plain Shore, near *Dunkirk*, when meaſured again, differed 3 Feet from the former Meaſurement; which is a much greater Difference than that Mr *Celſius* and the other Gentlemen found, in meaſuring a much longer Line twice over, which was but 4 Inches.

As to the Aſtronomical Obſervations taken by the 6 Foot Sector, whoſe Limb of $12°$ was divided only at every $20''$; it is true, M. *Caſſini* examined the Inſtrument ſeveral Ways at *Paris*, after his Return thither: but that a Correction, owing to the Change of Centre, might be ſafely applied to the Obſervations at *Dunkirk*, the Examen of the Centre ſhould alſo have been taken at *Dunkirk*; it being uncertain, whether this Alteration or Aberration of the Centre was cauſed by the Journey to or from *Dunkirk*.

The Difference of $41''$ between the Obſervations taken to ſettle the true Meaſure of the Arc of the Heavens, ſeems to be enormous. Perhaps the Stars were not lucid enough to be well obſerved by the 3 Foot Tube; but might they not, for a due Degree of Accuracy, have been viewed through the 9 or 10 Foot Teleſcope?

Our Author prefers the Obſervations of 1719, made after the Return to *Paris*, to thoſe made before; becauſe made at the ſame Time of the Year with thoſe of *Dunkirk*, and ſo not ſtanding in Need of Mr *Bradley*'s Correction: Though this Caution, perhaps, may be thought not neceſſary here, where the Errors of the Obſervations are greater than the Correction itſelf. Mr *Celſius* remarks farther, if the Difference of Latitude between *Dunkirk* and *Paris* be ſuppoſed to be $2° \; 12' \; 12\frac{1}{2}''$, which is a Mean between 4 others he mentions, the Length of a Degree will amount to but 56,395 Toiſes. And if the Obſervations at *Malvoiſine* and *Amiens* be counted, according to Mr *Bradley*'s Theory, for the Interval of a Month between the Obſervations, the Length of a Degree will come out to be 56,926 Toiſes; which is 135 Toiſes leſs than the Length of a Degree, found by meaſuring the whole Length of

of *France*; and 134 lefs than that of Mr *Picard*, fo highly approved of by M. *Caffini*, as confirming his own.

2. Mr *Celfius* having finifhed his Remarks upon the Obfervations made in the North Part of *France*, extending from *Paris* to *Dunkirk*, proceeds to examine thofe taken in the South, from *Paris* to *Collioure*, near the Borders of *Spain*, and the *Pyrenean* Mountains. By the former, a mean Degree was found to confift of 56,960 Toifes, by the latter 57,097; and confequently the Earth is an oblong Spheroid. *The fame continued.* Ibid. p. 378.

Mr *Celfius*, in examining thefe Obfervations, which were taken under the Conduct and Direction of the late M. *Caffini* in 1700, firft confiders the Structure and Goodnefs of the Inftruments ufed; then the Accuracy of the Aftronomical Obfervations for finding the Difference of Latitude; and, in the laft Place, the Trigonometrical Operations for determining the Diftances of Places; efpecially the two Extremes under the fame Meridian.

The principal Inftrument M. *Caffini* carried with him, was, a Limb of 12°, whofe Radius was indeed 10 Foot, but divided only into Degrees and Minutes; the other Parts were added to it at *Perpignan*. Here Mr *Celfius* obferves, that the finding the true Centre of this Limb was and ftill is a very difficult and troublefome Problem to a good Artift; that no Mention is made, whether the Pofition or Place of this Centre, and the Divifions of the Limb, were ever examined at *Paris* or *Collioure*, though the Carriage of the Inftrument through fo long and rough a Way, could not but make fome Alteration in the Place of the Centre.

It is true, the Zenith Diftance of *Capella*, taken by it at *Paris*, was confirmed to be right by another Inftrument; but it cannot be concluded, that the Zenith Diftance of the fame Star, taken at *Collioure* by this Inftrument, and not confirmed there by another Inftrument, muft be true alfo. For the Point of Divifion, anfwering to this Diftance in the Limb, was not examined; and a Centre wrong placed may by Accident give the true Zenith Diftance, *viz.* when the true and erroneous Centre happens to lie in the fame Perpendicular to the Horizon.

The Exceptions taken to the Aftronomical Obfervations for finding the Difference of Latitude between *Paris* and *Collioure*, are, in the firft Place, That though 5 Stars were obferved at *Collioure* and *Paris*, yet 1 only was made ufe of, *viz. Capella*: That the Difference of Latitude by *Capella* is 6° 18′ 57″: If *Lucida Lyra* had been ufed, the Difference would have been but 6° 17′ 7″; but by the Right Shoulder of *Auriga*, 6° 19′ 25″: Hence arifes the Uncertainty or Difference of 2′ 18″ between the greateft and leaft of their Obfervations: That the late M. *Caffini* makes the Difference 57″ lefs than M. *Caffini*, who accounts for this Difference from the Obfervations being taken by an ordinary Inftrument; but the Inftrument is the fame which was ufed to take the Altitude of the Pole of *Amiens*, which was very near that found by Mr *Picard*.

As to the Trigonometrical Operations for finding the Diſtance of Places, Mr *Celſius* thinks they labour under conſiderable Uncertainties; not only on the Account of the many Difficulties they met withal, *viz.* mountainous Countries, want of proper Signals, *&c.* ſo that convenient Triangles could not be formed; but add to all theſe, ſeveral of the Triangles had but Two Angles obſerved, and ſome of theſe Angles too acute; whence, as M. *Caſſini* himſelf very juſtly obſerves, in his Examination of *Snellius* and *Riccioli*'s Obſervations, great Errors may ariſe. M. *Picard* thinks all Angles leſs than 20 Degrees ought to be avoided; as alſo that the Triangles ſhould be contrived ſo as to have Sides of a due Length, neither too great nor too ſmall: Then follow 16 Triangles, wherein one or more of theſe Inconveniencies are to be found.

It may be ſaid, the Whole of theſe Obſervations and Meaſures of M. *Caſſini* ſeem to be ſufficiently confirmed, if not aſcertained; ſince the principal Baſe in *Rouſſillon* was found, when computed, to differ but Three Toiſes from the ſame as it was actually meaſured; and that, after ſome due Corrections, it was made to agree with the greateſt Exactneſs. Mr *Celſius* replies, Why are we not told what thoſe Corrections were, that we may ſee whether they were really neceſſary or no? Why were they not taken Notice of in the Calculations of each Triangle? Beſides, the real Length of the Baſe, or the fundamental Line, in *Rouſſillon*, is not fully aſcertained, it not being meaſured more than once; whereas that at *Dunkirk* and that of M. *Picard* were meaſured twice; and there was more Reaſon for doing ſo here than at *Dunkirk*, on account of the uneven and almoſt ever changing Shore in *Rouſſillon*, from the reſtleſs overflowing Sea.

The great Number of the Triangles, joined with the numerous ſmall Errors of the Angles, is another Ground of Uncertainty; for the Errors in the Angles, though ſmall, may make the Diſtance of the Parallels of the 2 extreme Places greater than it ought to be; and yet the principal Sides, that is, thoſe that are made Baſes to the following Triangles, continue the ſame. This made it neceſſary to verify the Sides, at leaſt at every ſecond Degree, by meaſuring the principal Baſe twice over with due Care; which might have been done, and therefore ſhould have been done, in a Matter of ſo much Nicety as an Attempt to find the Difference between Two Degrees ſo near one another, under the ſame Meridian.

To ſhew what bad Conſequences may ariſe from ſmall Errors committed in obſerving the Angles of ſeveral Triangles, Mr *Olavus Hiorter*, a curious and ingenious Friend of Mr *Celſius*, has taken the Pains to form the Triangles of M. *Caſſini* between *Bourges* and *Collioure*; ſo that the Diſtance between their Parallels ſhall be conſiderably leſſened; and yet the Baſe in *Rouſſillon*, found by Computation, ſhall not, after due Correction, differ ſenſibly, if at all, from the ſame actually meaſured. In conſequence of this, Mr *Celſius* concludes with obſerving,

that the Diſtance between the Royal Obſervatory and the Perpendicular to the Meridian of *Collioure*, deduced from the Triangles of *Caſſini*, corrected after Mr *Hiorter*'s Method, *&c.* will amount to but 358,980 Toiſes. This, divided by the mean Difference of their Latitudes, 6° 19′ 11″, will give 56,803 Toiſes, for the Length of a Degree, one with another, between *Paris* and *Collioure*, which is leſs than the Length of a mean Degree found by M. *Picard*, and pretty near the Truth: So that the Degrees decreaſe as you go towards the Equator; and conſequently the Earth is higher at the Equator than at the Poles, as Sir *I. Newton* and Mr *Huygens* believed.

The Diſtance of the Parallels of *Paris* and *Collioure* by this Method is indeed leſs than that computed by M. *Caſſini*; but this cannot reaſonably be complained of, ſince theſe computed Meaſures of M. *Caſſini* ſeem very capable of being leſſened; and it is no more than what M. *Caſſini* himſelf hath done to the Meaſures publiſhed by his Father, which he has ſhortened by 325½ Toiſes. But however that Matter be, whether this particular Correction of M. *Caſſini*'s Diſtance, and, conſequently, Length of a mean Degree, be admitted or no, Mr *Celſius* is fully perſuaded, upon the Whole, that he hath made it plain to every unprejudiced Reader, that theſe Two Sets of Obſervations in *France* are not taken with ſuch a Degree of Exactneſs as to be depended upon, in determining ſo nice a Matter, in Diſpute for 50 Years, as the true Figure of the Earth; which was the Thing propoſed to be done by them.

Concerning a Place in New-York *for meaſuring a Degree of Latitude, by Mr* J. Alexander. *Ibid. p.* 453.

IV. The Mention of the *French* Endeavours to diſcover the Figure of the Earth by Obſervation, puts me in Mind, That a very exact Obſervation for that Purpoſe might be made here, becauſe *Hudſon*'s River here is frozen over from *New-York* up to *Albany*, and it's Courſe is very ſtrait, almoſt true North, and the Diſtance between *New-York* and *Albany* is above One hundred and Fifty Miles; *New-York* is in Latitude of 40° 40′, nearly; ſo that the Length of above 2 Degrees of Latitude on the Earth might be meaſured here, with much more Exactneſs than it was poſſible in *England* or *France*, becauſe of the Aſcents and Deſcents, and curved Lines, which, I think, they would continually be obliged to make Allowances for.

From all which Difficulties the Menſuration here on the Ice would be entirely clear.

A Propoſal for the Meaſurement of the Earth in Ruſſia, *read at a Meeting of the Academy of Sciences of* St Peterſbourg, Jan. 21, 1737, *by Mr* Joſ. Nic. de L'Iſle, *firſt Prof. Aſtron.*

V. Neceſſity, or the Exigencies of Geography and Navigation, put Mankind very early upon the Enterprize of meaſuring the Earth. For how is it poſſible to conſtruct the Charts of each Kingdom or Empire, without ſetting down all the Places in their true Diſtances, by the Meaſures made uſe of in each Country: Such as were the *Stadia* of the Ancients, and ſuch as are our Miles, Leagues, Werſts, *&c.* And how could different States be compared with one another, ſo as to come at the Knowledge of the Spaces they ſeverally occupy on the Earth's Surface, without knowing the Number of theſe common Meaſures contained in a Degree, or in the whole Extent of the Earth? Hence proceeded the

and F. R. S. Translated from the French printed at St Petersbourgh, 1737, 4to. By T. S. M.D. F.R.S. No. 445. p. 27. Jan. &c. 1737.

the twofold Method of determining the Situation of the different Parts of the Earth, either by their mutual Diſtances ſet down in the Meaſures made uſe of in each Country, or expreſſed in Meaſures common to all, as Degrees, Minutes, and Seconds, by marking the Longitude and Latitude of each Place.

Upon the firſt Determination of the Magnitude of the Earth in Geographical Meaſures, as in *Stadia* and *Arabian* Miles, the Ancients did not employ any great Degree of Exactitude. They were content to ſet down the Circumference of the Earth, and of it's Parts, in round Numbers; probably, becauſe they did not expect to be able to attain much Preciſeneſs in a Reſearch of this Nature. But according as their Deſires of improving Geography increaſed, by entering into a Detail of it, they found it neceſſary to have a more exact Knowledge of the Magnitude of each Degree, not only in great Meaſures, as in Miles and Leagues, but alſo in Perches, Toiſes, and Feet; which could not be done otherwiſe than by Geometrical Operations and Aſtronomical Obſervations, more exact, and conſequently more operoſe, than had been, or indeed could have been, undertaken before.

I ſhall not enter here upon a Detail of the immenſe Labours of modern Mathematicians on this Head, as thoſe of *Fernel* in *France*; of *Snellius*, *Blaeu*, and *Muſſchenbroek* in *Holland*; *Norwood* in *England*; Father *Riccioli*, and lately *Monſignor Bianchini* in *Italy*; and the Gentlemen of the *Academy of Sciences* in *France*; to get only the preciſe Magnitude of a Degree in the Meaſures of their reſpective Countries. But I will anſwer an Objection which might be raiſed hereon, *viz.* That it was needleſs to undertake theſe ſame Operations in ſo many different Places, ſince the Magnitude of a Degree once determined in the Meaſures of any one Country, may be eaſily reduced to the Meaſures of any other, by the exact Knowledge we now have of the Proportions of modern Meaſures. Whence it might be inferred, that after all the Exactneſs which the Aſtronomers of the *Royal Academy of Sciences* of *Paris* have obtained by their Labours, in drawing their Meridian from one Sea to the other, it is unneceſſary to enter upon a new Undertaking of the ſame Thing any where elſe: Since, in order to reap the Advantage of that Work for the Geography of each particular Country, nothing more is requiſite than exactly to compare the Meaſures of thoſe Countries, with thoſe made uſe of by the *French* Aſtronomers in their Operations and Calculations.

Now, taking *Ruſſia* for the Example, the Geographical Meaſures of which are *Werſts*, divided each into 500 *Sagenes*, and each *Sagene* ſuppoſed to be exactly ſeven Feet *Engliſh*; this Relation once known, as alſo the exact Relation of the *Engliſh* to the *French* Foot, or to the Toiſe of ſix Feet, which the *French* Aſtronomers employed in their Meaſurements, and of which they found a Degree of a great Circle contained 57060; what more is requiſite for concluding that a Degree of a great Circle contains 104½ *Werſts*? And what remains towards the Perfection

fection of the Geography of *Ruſſia*, in the moſt minute Detail that can be entered upon, but to employ this Meaſure of *Werſts*, *Sagenes*, and *Engliſh* Feet, (if you pleaſe) in actual Meaſurements; and to conſtruct the Charts by the moſt exact Methods of Geometry; taking care to ſet them down right, as to their true Bearings, and to regulate them by the moſt exact Aſtronomical Obſervations of Longitude and Latitude that can poſſibly be made.

It muſt be confeſſed, we ſhould be very happy, if in the Geography of *Ruſſia* we were arrived at this Pitch; not only in the general Map, but likewiſe in that of any particular Diſtrict whatſoever, the neareſt and of moſt Concern to us. But beſides that we are as yet far from pretending to this; I will now make appear that it is not poſſible to attain it, without undertaking an equal and even a greater Work than all that has been hitherto done in *France* and elſewhere, towards the Meaſurement of the Earth. I am myſelf affrighted at the very Thought of what I propoſe, and am under Apprehenſions that it will give the ſame Pain to thoſe of the Company, who know, as well as I, the prodigious Labour in which this Work muſt engage the Undertakers. But what is not a Perſon capable of undertaking for the Glory and Intereſt of her Imperial Majeſty, when excited by the Benefits ſhe heaps on the Academy, and by the ſingular Protection her Miniſters grant to this Body and the Sciences therein cultivated! Sufficient Motives for undertaking Matters of the utmoſt Difficulty.

When I ſaid above, that an exact Knowledge of the Magnitude of a Degree of the Earth in any known Meaſures of one Country was ſufficient for conſtructing exact Charts of all other Countries, only having a Regard to the different Proportion of the Meaſures; that is to be underſtood upon a Suppoſition of the Earth's being perfectly ſpherical: Seeing it is well known, that in a Sphere the Degrees of all the great Circles are every where equal; and that we likewiſe know, in a Sphere, the Proportion of the Degrees of the ſmall Circles to their great Parallels; according to their Diſtance from them.

But if the Earth be not perfectly ſpherical, the Caſe is quite altered: All the Degrees of the great Circles will not be equal to one another; and thoſe of the ſmall Circles, taken at a certain Diſtance from their parallel great Circles, will not have the ſame Relation that the Degrees of the ſmall Circles, taken at the ſame Diſtance, would have on a Sphere. In all this there might poſſibly ariſe an infinite Variety, according to the Figure the Earth might have; and as it is not yet decided what is the Earth's true Figure, and that there is no better Method of aſcertaining it than by Obſervations made in ſo great an Extent as that of *Ruſſia*: For theſe Reaſons I have advanced, that the Perfection of the Geography of *Ruſſia* ſtands in Need of this great Undertaking; which, beſides the Uſefulneſs of it, will acquire much Honour to the Academy of *Petersburgh*; if that Body can, by Means of this Work, contribute towards the deciding the celebrated Queſtion of the Earth's

Figure. But before I enter into a Detail of the great Advantages of this Refearch, and the Nature of the Operations I propofe, it is neceffary to explain in what Manner I mean that the Queftion of the Earth's Figure and Magnitude is not yet decided.

There have been fome who have long fince fufpected, and even thought they were furnifhed with Proofs of the Earth's not being exactly fpherical. I here entirely abftract from the Unevenneffes of it's Surface, which are not fenfible in regard of the Earth's whole Bulk; feeing the Tops of the higheft Mountains, and thofe even few in Number, are fcarce more than a League above the Level of the Seas. Wherefore, I fuppofe the Earth to be bounded by a Curve Surface, fuch as it would be by the Level of the Sea carried quite over all the Earth. It is in this Manner, the Earth being confidered as covered with a Fluid, that Sir *Ifaac Newton*, in the firft Edition of his *Principia*, publifhed in 1686, has demonftrated, that fuppofing this Fluid homogeneous, and the Earth to have been at Reft at the Time of it's Creation, it muft have affumed the Figure of a perfect Sphere: But afterwards, fuppofing it to have a Motion on it's Axis, as is well known it has in 24 Hours; this fpherical Figure muft have been changed into that of a Spheroid, flatted at it's Poles, in which the Degrees on the Meridian muft be greater drawing near the Poles, than near the Equator.

Sir *Ifaac* confirms this Hypothefis of the Earth's Figure, by Obfervations of the Diminution of the fimple Pendulum upon approaching the Equator: To which Dr *Pound* adds the Analogy the Earth has with fome of the other Planets, as *Jupiter*, which fometimes appears oval, it's leaft Axis being that about which it makes it's Revolution.

This Opinion of Sir *Ifaac* has likewife been maintained by Mr *Huygens*, though with fome fmall Difference. But in 1691, Mr *Eifenfchmid* * having compared the Meafurements of the Earth made in different Latitudes, as that of Father *Riccioli* in *Italy*, of M. *Picart* in *France*, and of *Snellius* in *Holland*; and having found that the Degree, which refulted from thofe different Meafurements, continued to grow lefs in drawing nearer the Poles, (which is quite the contrary of what follows from the Earth's Figure fuppofed by Sir *Ifaac* and *Huygens*) Mr *Eifenfchmid* was thereupon of Opinion, that the Earth was longer at the Poles.

This Opinion of Mr *Eifenfchmid* was afterwards confirmed by the late M. *Caffini*, in the Obfervations of the Meridian of *Paris*. For in 1701, having carried on thefe Operations to the *Pyrenæan* Mountains, which is a Space of above $7\frac{1}{2}°$, he found, that as he advanced to the South thefe increafed $\frac{1}{800}$ Part, or 72 Toifes each Degree.

* Jo. Cafp. Eifenfchmidii Diatribe de figura telluris Elliptico-Sphæroide; ubi unà exhibetur ejus magnitudo per fingulas dimenfiones, confenfu omnium Obfervationum comprobata. *Argentorati, apud Joh. Frider. Spoor.* 1691. *4to. (pag.* 54. *cum fig.)*

Since

Since the Meridian of *Paris* was, in 1718, carried on Northward to the Sea, M. *Caſſini*, the Son, found, upon comparing more than 8°, which this Meridian contains from Sea to Sea, that the Increaſe, going Northward, was but from 60 to 61 Toiſes each Degree; as may be ſeen in the large Treatiſe publiſhed in a ſeparate Volume, as a Sequel to the Memoirs of the *Royal Academy of Sciences* of *Paris* for the Year 1718. Theſe Reaſons did not hinder Sir *Iſaac* from perſiſting in his firſt Opinion of the Figure of the Earth flatted at the Poles, as appears in the 2d and 3d Editions of his *Principia*, publiſhed in 1713 and 1726: And it is very ſurprizing, that by this very Figure of the Earth he demonſtrates a certain Motion it has, to explain in the *Copernican* Syſtem the Preceſſion of the Equinoxes, or the apparent Motion of the fixt Stars in Longitude. Sir *Iſaac* finds the Inequality of the Degrees on the Meridian, in ſo little an Extent as that of *France*, not ſenſible enough to be poſſibly determined by immediate Obſervations; and he is of Opinion, that we ought more to rely on the Obſervations of the ſimple Pendulum, and on the other Principles which he has built upon, to conclude the Earth flatted at the Poles.

In 1720, M. *Mairan* attempted to reconcile the two different Hypotheſes of Sir *Iſaac* and M. *Caſſini*, by imagining that the Earth, at it's Creation, being without Motion, was of a much more oblong Figure than that which *Caſſini* thinks it has at preſent; ſo that it might have been reduced to that which it now has, by the diurnal Motion on it's Axis, *&c.* But Dr *Deſaguliers*, who is of Sir *Iſaac*'s Opinion, has made appear, that M. *Mairan*'s Suppoſition is contrary to the Laws of Motion; and has moreover propoſed ſeveral conſiderable Doubts on the Obſervations and Suppoſitions employed by M. *Caſſini* in his Determination of the Earth's Figure in 1718.

As ſoon as the Meridian of *Paris* had been extended from one Sea to the other, and M. *Caſſini* had thence deduced a Confirmation of the Syſtem of the Earth's being longer at the Poles; I imagined a new Method of deciding the Queſtion, by the Obſervation of the Degrees of the Parallel compared with thoſe of the Meridian.

For that Purpoſe I conſidered, that as the Degrees of the Meridian and thoſe of the Parallel, at the ſame Elevation of the Pole, had different Relations, according to the different Figures aſcribed to the Earth; nothing more was requiſite for concluding which Hypotheſis was the true one, than to determine this Relation by immediate Obſervation.

Having ſuppoſed, that there had been obſerved on the Parallel of *Paris*, a Space nearly of the ſame Magnitude with that on the Meridian, that is, of about 13 Degrees, ſince that on the Meridian is about $8\frac{1}{2}$°; I found by an exact Calculation, that according to the Figure which M. *Caſſini* has given to the Earth, this Space ought to contain $13\frac{1}{2}$ ′ of the Parallel more than in the Hypotheſis of the Earth's being ſpherical; which appeared to me conſiderable enough to be able to decide between theſe two Hypotheſes, and by a ſtronger Reaſon between the

the Hypothefes of *Newton* and *Caffini*; feeing the Difference ought to be ftill more confiderable than that now fpecified.

I concluded, at leaft, that, independent of the Figure of the whole Earth, which could not be determined by the fole Obfervations made in *France*, without making Suppofitions, and admitting Principles, which are ftill liable to be contefted; it would be of great Confequence towards conftructing exact Charts of the Kingdom, to afcertain this Relation by Obfervations, which confifted only in forming Triangles along the Parallel of *Paris*, and obferving at the two Ends the Difference of the Meridians, by the moft exact Methods.

The Difference, which I have now mentioned, feemed to me to be fo confiderable, that I was in hopes of being able to determine it by Means only of two Places within Sight of one another, and fituated to the Eaft and Weft; provided their Difference of Longitude were accurately obferved, independently of Aftronomical Obfervations, by Means of lighted Fires; after the Manner that M. *Picart* put in Practice in *Denmark*, for determining the Difference of Longitude of the Aftronomical Tower at *Copenhagen*, and of *Uraniburg* in the Ifle of *Huen*. With this Intent, in *April* 1720, I went fome Diftance from *Paris* Southward, to the Places which I judged moft proper for my Purpofe; but my Defign was not then executed, for Want of Affiftance, and for other Reafons, which I fhall pafs in Silence.

Since that Time, I faw with Pleafure, that the Marquis *Poleni* had hit upon the fame Thought with me; as may be feen in his Letter to the Abbot *Grandi*, dated *Nov.* 1724.

The Decifion of this famous Queftion of the Earth's Figure had ftopped there, when in the Year 1733, the Minifter of *France* having thought it neceffary to conftruct an exact Map of the whole Kingdom; and being informed, that the Work could not be better carried on than by the Aftronomers of the *Royal Academy of Sciences*, applied to M. *Caffini* on that Head; who was of Opinion, that, in order to execute it with the utmoft Exactitude, the fame Method ought to be employed as for the Meridian, by taking through the whole Extent of the Kingdom, Triangles linked together by Means of Objects feen fucceffively one from another, &c. This Project of making a Map of *France* by fuch Triangles, had been already offered to M. *Colbert* by M. *Picart* in 1681, but was not then executed. However, M. *Caffini* propofed, that thefe Triangles fhould be begun in a Direction perpendicular to the Meridian; in order to render thefe Operations of Service towards the Decifion of the Earth's Figure, purfuant to the Method which I fpoke of above: And M. *Caffini*, having in Perfon undertaken thefe Operations, and having carried them that fame Year, 1733, from *Paris* to *St Malo*, whofe Longitude from *Paris* M. *Picart* had obferved in 1681; the Relations of the Degrees on the Meridian and Parallel were found to be fuch as were required in the Hypothefis of the Earth lengthened at the Poles, and even more lengthened than *Caffini* had

determined

determined in 1718. For inftead of the Diminution of $\frac{1}{60}$ Part for each Degree of the Parallel, which I had found according to the Earth's Figure, as determined by *Caffini* in 1718, he deduced from his Operations in 1733, a Diminution of the 36th Part of each Degree.

True it is, that M. *Caffini*, in the Account he gave of this Determination at the publick Meeting of *Nov.* 14, 1733, does not give it as entirely fure; becaufe the Longitude of *St Malo*, with regard to *Paris*, was collected but from one Obfervation only of *Jupiter*'s firft Satellite, wherein there may poffibly be fome Error: But at leaft M. *Caffini* feems certain, that there is a very confiderable Diminution in the Degrees of the Parallel of *Paris*, which confirms his Opinion of the Earth's being longeft at the Poles. This we are likely to have a better Certitude of hereafter, feeing we are informed that this Meafurement of the Parallel of *Paris*, is carrying on in *France* by M. *Caffini*'s Sons, M. *Maraldi*'s Nephew, and feveral other young Mathematicians, inftructed by M. *Caffini* in this fort of Work.

I have already faid, that all thefe Operations performed in *France*, for the Figure and Magnitude of the Earth, could not ferve to determine the Earth's Figure out of *France*, without the Affiftance of certain Hypothefes; unlefs the fame thing were undertaken and carried on in the other Regions of the Earth, more Northern and Southern than *France*. 'Tis upon this Confideration, that the *Royal Academy of Sciences* took up the Refolution of fending fome Aftronomers to make the like Obfervations as near the Equator and the Poles as poffible, which are the Places where the difference of the Degrees on the Meridian ought to be the greateft, according to the different Hypothefes.

In *April* 1735, fet out from *France* 3 Mathematicians and Aftronomers of the Academy, *viz.* Meffieurs *Godin*, *Bougher*, and *De la Condamine*, for the Province of *Quito*, which is the moft Northern part of *Peru* in *America*; in order to obferve, juft under the Equinoctial Line, the Magnitude of fome Degres of the Meridian and Equator.

As to the other Mathematicians and Aftronomers of the fame Academy, *viz.* Meffieurs *de Maupertuis*, *Camus*, *Clairaut* the Son, and *Monnier* the Son, who have been fent to the North, they departed from *France* in *April* 1736, with Mr *Celfius* Profeffor of Aftronomy at *Upfal*, who accompanied them to *Sweden*, as far as the Bottom of the Gulph of *Bothnia*, where they might meafure about a Degree on the Meridian at it's croffing the Polar Circle. But as, by the laft News I received from them, they had not finifhed their Operations, 'tis not yet known whether the Magnitude of the Degree meafured by them, favours the Opinion of M. *Caffini*, or that of Sir *I. Newton*. All we know is, that they have found the length of the fimple Pendulum favourable to the latter, that is, longer under the Polar Circle than farther South. My Brother *De la Croyere*, had already found the fame Thing: For being at *Archangel* in 1728, he there obferved, in the moft exact Manner he poffibly could,

the Length of the ſimple Pendulum, which he found to be $\frac{3}{20}$ Parts of a Line longer than at *Paris.*

We are likewiſe informed by the other Aſtronomers gone to *Peru*, that in their Way towards the Equator, being at *St Domingo*, in the Latitude of 18° 37′, they there found the Pendulum ſwinging Seconds, to be about two Lines ſhorter than at *Paris*. Thus, all we as yet know from thoſe Gentlemen, on the Expeditions to the North and the Line, confirms the Opinion of Sir *I. Newton* and his Adherents: And yet M. *Mairan*, whom I have already mentioned, pretends, that this ſhortening of the Pendulum in drawing nearer the Equator, is in one Senſe entirely independent of the Earth's Figure.

Thus it appears from the foregoing Account, that the Queſtion concerning the Earth's Figure is not yet at an end. Nay, 'tis not impoſſible, that after finiſhing all the Obſervations which are actually making, new Difficulties may ariſe, and new Objections be ſtarted, that may prevent it's being entirely decided. However, all this Work cannot fail giving great Light to this important Queſtion, and procuring conſiderable Advantages to Geography, Aſtronomy, and Natural Philoſophy.

'Tis with this View, and particularly to render ſuch important Service to the Geography of *Ruſſia*, that I think it neceſſary to undertake a Work of that Nature in *Ruſſia*; towards executing which we have great Advantages, which the other Nations have not. One of the principal of theſe Advantages is the great Extent of *Ruſſia* every way. For were the Meridian of the *Imperial Obſervatory* of *Petersbourg* to be determined, it might be carried to between 22 and 23 Degrees; which is a fourth Part of the Diſtance from the Pole to the Equator. The Meridians of *Moſco* and *Aſtracan* are not of leſs Extent; and conſequently we might, by the Meaſurement of ſome one of theſe Meridians, determine more exactly than could have hitherto been done, the Inequality that ſubſiſts between the Degrees of the Meridian.

This is what the great *Caſſini* wiſhed, when, after having, in 1701, determined this Inequality by the Extent of 7° obſerved in *France*, as has been mentioned above, he ſays, that this Fact might be verified by Menſurations of greater Extent, if the other Princes of the Earth did contribute as much as the *King of France* towards the perfecting of Sciences.

M. *Caſſini* was then ignorant of the Views which *Peter* the Great had formed in the Eſtabliſhment of the *Academy of Sciences* at *Petersbourg*; nor could he then foreſee that her preſent Imperial Majeſty, was deſtined not only to purſue the Deſigns projected by that great Monarch, but alſo to ripen them to Perfection, by granting ſuch Succours and Aſſiſtances for the Promotion of Science, as were never yet afforded from any of the greateſt Princes of the Earth.

In the great Extent which might be given to the Meridian of *Petersbourg*, as abovesaid, there would be the Advantage of knowing, by Operations linked together, or uninterrupted, the Magnitude of ſome Degrees equal to thoſe which have been meaſured in *France*, and to that which

which the *French* Aſtronomers have meaſured in *Sweden*; and not only all the Degrees between the two, which the *French* Aſtronomers have not had in their Power to obſerve, but alſo ſome Degrees farther Northward than that meaſured by them in *Sweden*.

As the Exigencies of Geography require the Triangles, taken for the Determination of the Meridian, to be continued on every Side, and principally in Directions perpendicular to the Meridian, or according to the Parallels; with how great Exactitude may we not then determine the Proportion of the Degrees on the Parallels to thoſe on the Meridian, by means of the vaſt Extent of the *Ruſſian* Empire, which on it's Weſtern Side extending as far as all the Dominions of *Europe* from the moſt Northern to the moſt Southern, has no other Bounds to the Eaſt than the Eaſt itſelf, if I may be indulged the Expreſſion; ſeeing it's Extent that Way contains near half the Earth?

Another great Advantage to be obtained by the Work I now propoſe to be made in *Ruſſia*, is, That we, coming after others, ſhall reap the Benefit of all their Knowledge and Experience in the like kind of Meaſurements: Whence we may expect to ſucceed and execute it better than could have been done elſewhere, by applying timely Remedies againſt the Difficulties that occurred in other Places.

Theſe Operations are to be founded on a Baſis of the greateſt Length poſſible; which muſt be actually meaſured, and with the greateſt Exactneſs that may be; as it is to ſerve for a Foundation to the Meaſurement of all the Triangles. And in this Point too we have a very great Conveniency near *Petersbourg*, ſeeing on the Ice here we may meaſure out a Baſis, greater than has been hitherto taken, namely, from the Coaſt of *Ingria* about *Peterhoff*, to the Coaſt of *Finland* toward *Syſterbeck*. There is not leſs than 20 Werſts Diſtance between theſe two Extremities, and this great Diſtance may be meaſured very exactly, this Year eſpecially, that the Ice is very even. Moreover, as this Baſis is ſituated between the Iſle of *Cronſtad* and *Petersbourg*, in a Direction nearly perpendicular to the Diſtance from *Petersbourg* to *Cronſtad*, there can be no better Method for inferring thence, by exact Obſervation of the Angles taken at the Extremities of this Baſis, the Diſtance from the Centre of the Imperial Obſervatory to the Steeple of the new Church of *Cronſtad*; which two Objects are ſeen reciprocally from each other, and are not leſs than 30 Werſts aſunder: And this Diſtance once known exactly, will ſerve as a Foundation for all the Triangles that are to be taken; of which each of the Sides may have not leſs than from 30 to 40 Werſts, according as we ſhall find Objects advantageouſly ſituated for that Purpoſe. We have, to begin with, the Mountain of *Douderhof*, which, with the Imperial Obſervatory, and the Steeple of *Cronſtad* Church, forms one of the moſt convenient Triangles imaginable for the Subject we propoſe.

In taking Obſervations at theſe three Places, we ſhall ſee if we can diſcover others of the ſame advantageous Situation; but when no

remarkable Objects are found of the Situation and Diſtance ſought for, they muſt be erected on purpoſe, in the ſame manner as was of neceſſity done in other Countries: And this may be done here with more Eaſe, ſeeing, in Places where the Woods intercept our Sight, ſmall Towers may be raiſed, at very little Expence, out of theſe ſame Woods, with Signals placed on them, which may be ſeen as far as may be required. In open Places, where conſequently Wood is not ſo common, Signals alone, without Towers, will ſuffice.

The moſt neceſſary Inſtruments for executing this undertaking, are, beſides the ordinary Aſtronomical Inſtruments, a common Quadrant of between 2 and 3 Feet *Radius*, for obſerving the Angles of the Triangles that ſhall be taken; and a Portion of a Circle of the greateſt Radius that can be conveniently had, for obſerving the Arches of the Heavens correſponding with the Diſtances meaſured on the Earth.

I ſay, the Quadrant ought not to have a Radius of more than between 2 and 3 Feet: For if it be bigger, it cannot for the moſt part be made uſe of in Steeples and other Places of conſiderable Height, where 'tis requiſite to obſerve; but alſo if it be leſs than 2 Feet, it will not give the Value of the Angles with ſufficient Exactneſs.

As to the other Inſtrument for obſerving the Arches of the Heavens, it's Radius ought not to be leſs than from 12 to 15 Feet: but 'tis not neceſſary that it ſhould contain a large Portion of a Circle. 'Tis only requiſite to have this Portion ſomewhat larger than the Arch of the Heavens intended to be meaſured. Thus, as the Meridians, which may be traced in *Ruſſia*, can be extended but between 22° and 23°, as already mentioned, it will ſuffice, that the Inſtrument employed therein be a Portion of a Circle of 30°.

M. *Picart*, for his firſt Operation, got an Arch of a Circle made of 18° and of 10 Foot Radius, with which he thought himſelf ſure within 2″ or 3″: And no other Inſtrument was made uſe of in the chief Obſervations for the Meridian of *Paris*. The Aſtronomers who are gone to *America*, carried with them an Inſtrument of 12 Feet Radius, and of a Portion of a Circle of 30°. But thoſe come to *Sweden*, contented themſelves with a Portion of a Circle of 5½°, and 9 Feet Radius: But this Inſtrument, made by Mr *George Graham*, a very able *Engliſh* Mechanician, is by it's Conſtruction ſo exact, that the Aſtronomers who have uſed it, think themſelves ſure to 2″. The one we want for the Obſervations in *Ruſſia* ought to be made by the ſame Artiſt, and of the ſame Conſtruction.

'Tis with ſuch an Inſtrument that Mr *Bradley*, a celebrated *Engliſh* Aſtronomer, has diſcovered, in the Meridian Altitudes of ſome fixt Stars, certain conſtant and annual Variations, which do not proceed either from the Variation of the Refractions, or from the Parallax of theſe Stars, or, in fine, from any Nutation or Wavering of the Earth's Axis; but which he accounts for by the ſucceſſive Motion of Light.

Whatever

Whatever be the Caufe of thefe Variations, (which Caufe, as well as it's Effect, are not as yet, perhaps, entirely cleared up), as they may poffibly happen in the Space of Time requifite to be fpent in making the Obfervations for the Meridian, or in paffing from one End of the Meridian to the other; it is neceffary, with the fame Inftrument, or fuch another, that is of pretty near the fame Exactnefs, to examine the Variations of the Stars made ufe of: Wherefore it would be of confiderable Advantage, not only for the Obfervations of the Meafurement of the Earth, but alfo for all the other principal Refearches in Aftronomy, to have Orders given for procuring two mural Quadrants of Mr *Graham*'s Make, and of the fame Conftruction, as I have already fpecified; for which there are Walls already raifed at the Imperial Obfervatory, in the Plane of the Meridian.

With thefe two Quadrants, which might be of 7 Feet Radius, and the moveable Telefcope 9 or 10 Feet long, we fhould be in a Condition to make Obfervations of the utmoft Accuracy, fuch as the prefent State of Aftronomy requires.

Befides thefe Inftruments now mentioned, which are of abfolute Neceffity to a folid Eftablifhment of Aftronomy and Geography in this Country, there are ftill fome other fmaller Inftruments, that may be of great Ufe in the Operations I propofe, or may ferve to make other curious and ufeful Obfervations at the fame time that thofe for the Meafurement of the Earth are making.

When the Sides of the Triangles, taken for meafuring the Earth, terminate at very elevated Places, as on the Tops of the higheft Mountains, it is neceffary to reduce thefe Triangles to what they would be, had they been obferved in horizontal Plains fituated upon a Level with the Sea. For this Purpofe, we muft know the Height of the Mountains above the Sea's Level, which cannot always be determined geometrically, or would at leaft be too tedious to perform: Wherefore, in the Meridian of *Paris*, which croffed very high Mountains, M. *Caffini* was of Opinion, that he ought to fix their Height by a fhorter Method, which is that of the Height of the fimple Barometer, obferved on the Top of each Mountain, and compared with that obferved at the fame Time in another Place, whofe Elevation above the Sea's Level was known. But as that Method fuppofes the Knowledge of the Proportion which the different Fallings of the Mercury keep with the diffierent Heights to which the Barometer is carried; and as Natural Philofophers are not as yet entirely agreed on this Head, for want of Obfervations of fufficient Accuracy: Thence it happened, that Dr *Defaguliers*, making appear that M. *Caffini* has not employed the moft exact Proportion, found Reafons for correcting, or at leaft for doubting, of fome of M. *Caffini*'s Calculations. Thus it muft be by the Affiftance of new Experiments, better circumftanced than thofe hitherto made, and purfuant to a Theory entirely agreeing with thefe Experiments, that this Method may be employed with Certainty, for determining the Height of Mountains by the Barometer, and reducing the

the Angles obſerved from the Tops of theſe high Places, to what they would be, if they had been obſerved on a Plane horizontal with the Level of the Sea. Now theſe new Obſervations can be made on our Way in tracing the Meridian; and for that Purpoſe I have begun to conſtruct compound Barometers, which, by their peculiar Make, being very nice, will ſerve to obſerve with Accuracy the Quantity of the Mercury's Fall, at the different Elevations to which they ſhall be carried, in order to fix with greater Certainty the Proportion of that Fall. I ſhall take particular Care in the Conſtruction and Uſe of theſe Inſtruments to provide a Remedy againſt the Effect of Heat, which, as it is different in the different Times and Places of making theſe Experiments, may poſſibly produce apparent Variations, of which 'tis neceſſary to keep an Account.

There is ſtill another Method of determining the Elevation above the Level of the Sea of all the Points, in which the Triangles terminate, that are made for the Meaſurement of the Earth. This may be done by beginning theſe Operations near the Sea, as I propoſe to do, and actually meaſuring how many Toiſes and Feet the Places of the firſt Stations are elevated above the Level of the Sea. For if the Angles of the apparent Elevations of the ſecond Stations ſeen from the firſt be afterwards obſerved, it will be an eaſy Matter, from the known Diſtances, to deduce the true Elevations of the latter above the former, and conſequently above the Sea's Level, making proper Allowances in the Calculations for the Difference of the apparent Level from the true one. In this Method nothing is to be apprehended but the Variation of Refractions; but for this a Remedy may be found, for the moſt part, by returning upon one's Steps, that is, by reciprocally obſerving the firſt Stations ſeen from the ſecond: For if it be found, that as much as the ſecond Station appears elevated above the firſt, ſo much the firſt is depreſſed below the ſecond, except the ſmall Difference which muſt ariſe according to the given Diſtance, it will be a Proof, that the Refraction has been of no Prejudice.

The Obſervations and Determinations of the true Heights of all the Places which are to be viſited, will not be the leaſt laborious of thoſe that are to be made in theſe Journies; but then their Uſefulneſs will be a ſufficient Recompenſe for the Trouble; ſeeing they will afford us the Means of knowing all the chief Unevenneſſes of the Ground traverſed by theſe great Triangles, which being compared with the Length of the Courſe of the Rivers, may give us room to judge of their Rapidity, of the Eaſe or Difficulty of their Communications, *&c.*

The other conſiderable Obſervations and Experiments to be made in the Journies undertaken for ſuch Enquiries, are, the Obſervations of the Magnetic Needle, both as to it's Dip and Variation: But chiefly the Obſervations of the Length of the ſimple Pendulum, which, at preſent, is become requiſite to be obſerved with as much Exactneſs, and in as many Places as is poſſible; but alſo for which there are new Methods invented

invented, that we are promifed the Communication of, and which probably furpafs thofe hitherto made ufe of; in as much as, fince thofe Methods have been found by the *Royal Academy of Sciences* of *Paris*, it was thought proper to notify them to the Aftronomers fent to *Peru*, in order to put them in Practice in their Obfervations.

Whereas all thefe Operations and Obfervations, which I have here propofed, however arduous and difficult they may prove, have no other End than the Benefit of Geography; thofe who are to have the Management of this Enterprize muft be attended by feveral Surveyors and other Mathematicians of this Nation, who are to be inftructed on the Road, and employed at the fame Time in leffer Operations with fmaller Inftruments: By which Means the Maps of the Countries, taken in by thefe great Triangles, may be verified; and thus, according as this Work advances, the finifhing Stroke may be given to the Charts of *Ruffia*.

The actual Menfuration of the Bafis propofed in the preceding Article, by M. de L'Ifle. *Tranflated from the* French *by* T. S. *M. D. F. R. S. Ibid.* p. 50. *dated* Peterfbourg, May. 14. 1737.

VI. Since my laft, I undertook to meafure the Bafis fpoken of; and had the good Fortune to meafure very exactly on the Ice, by taking the precife Diftance between her Imperial Majefty's Caftle at *Peterhoff*, and the Caftle of *Doubki*, fituated oppofite to it, on the Coaft of *Finland*. I found the Diftance between the oppofite Walls of thefe Caftles to be 74,250 Feet *Englifh*. This Bafis, being much greater than any of thofe employed hitherto for this Purpofe, gives room to expect great Exactnefs in the whole Work, when it fhall be carried on in the fame Manner. It will at once ferve to make a very exact Map of the Bottom of the Gulph of *Finland*. 'Tis for the fame Defign, and for better ordering the Charts of the Coafts of the *Baltick*, that I intend (as foon as my Project fhall be approved here in it's full Extent) to begin to meafure my Triangles along the Coafts of *Ingria* and *Livonia*, to the Iflands of *Dagho*, *Oefel*, *&c.* And to the end that the Charts of the Places taken in by thefe Triangles may be finifhed at the fame Time, I fhall take with me all the Charts of thefe Parts, which can be had, in order to verify and correct them in my Way. According as thefe Charts are thus finifhed in the beft Manner, they may be engraved. I likewife intend to publifh, as foon as poffible, all the Operations and Obfervations I fhall have made in my Expedition; that thus early Benefit may be reaped from them, and that the Publick, at the fame time the Charts come out, may be acquainted with the Foundation on which they are conftructed. I once thought to have by this Time printed the whole Detail of my Operations in taking the Bafis, that is, of the Precautions I ufed in afcertaining it; but as it was meafured in *Englifh* Feet, which I have a Defire to reduce to this Country Meafure, and that 'tis requifite to confult the original Standards here on this Head, which I have not as yet been able to procure; for thefe Reafons, I am obliged to delay the Publication of thefe firft Obfervations.

An Account of an Improvement on the Terreftrial

VII. The Globe is juftly reckoned very ufeful and inftructive, both as a general Map, and alfo for explaining the firft Principles of Geography, and the fpherical Doctrine of Aftronomy. By this Inftrument it is eafy to

Globe, by Joseph Harris, *Gent.* N°. 456. p. 321. Jan. *&c.* 1740.

to find the Length of the Days, and their Increase and Decrease, in all Places, and at all Times of the Year. But this is not usually performed in such a manner as at the same Time to explain how these *Phænomena* arise from the Motion of the Earth, which is the principal thing Beginners especially should have in View: Nor can this be remedied, at least but in few Cases, as Globes are commonly fitted up; for the Axis and the horary Circle prevent the Brass Meridian from being moveable quite round in the Horizon, which it ought to be, and so indeed prevent the Globe from being universally useful, even in the common way of considering it.

It is now about 6 Years since I removed this Impediment, by placing two horary Circles under the Meridian, one at each Pole. These Circles are fixed tight between two Brass Collars placed about the Axis, but so that they may be easily turned by the Hand when the Globe is at Rest; and when the Globe is turned, they are carried round with it, the Meridian serving as an Index to cut the horary Divisions. The Globe, being thus fitted, serves readily for solving of Problems in South as well as in North Latitudes, as also in Places near the Equator. But the chief Advantage gained by this Alteration, is, that the Globe is now adapted for solving of Problems upon the Principles of the *Pythagorean* System, or to shew how the Vicissitudes of Days and Nights, and the Alterations of their Lengths, are really made by the Motions of the Earth. To expedite this, I had the Brass Meridian at one of the Poles divided into Months and Days, according to the Sun's Declination, reckoning from the Pole. This being done, if we bring the Day of the Month to the Horizon, and rectify the Globe according to the Time of the Day, the Horizon will represent the Circle separating Light and Darkness, and the upper Half of the Globe, the illuminated Hemisphere, the Sun being in the Zenith.

While we view the Globe in this Position, we see the Situations of all Places in the illuminated Hemisphere, with respect to the Horizon, Meridian, *&c.* and by observing the Angles which the Meridians, cutting any Parallels of Latitude in the Horizon, make with the Brass Meridian, we have the Semidiurnal Arches of these Parallels respectively: And at the same Time (if the Sun be not in the Equator) we see why the Diurnal Arches of the Parallels continually decrease from the Neighbourhood of the elevated Pole, till we come to the opposite Part of the Horizon. If we turn the Globe Easterly round it's Axis, we shall see how all Places change their Positions with respect to the general Horizon, the Meridian, *&c.* by the Motion of the Earth round her Axis.

It yet remains to be shewed, how the annual Motion of the Earth in her Orbit, causes the Change of the Sun's Declination: This cannot be done by the Globe simply taken, but is very well shewed by the Instruments called *Orreries*: But to these their Costliness is an Objection, not mentioning others from a want of due Proportion in the things they exhibit. I had therefore an Instrument made, which consisted only of a

round Trencher of Wood, a Circle of Brass upon the Face of it, and between these 3 Wheels of the same Dimensions and Number of Teeth: The innermost Wheel was fixed to the Wood in the Centre, the third had it's Axis come through the brass Plate, round which was a brass Circle having a Socket making an Angle with it of 66 ½ Degrees; in this Socket was fixed the Axis of a little Globe, having a Horizon about it, to represent the Circle separating Light from Darkness, the Sun being supposed to be in the Middle of the Instrument. While the brass Plate is turned round through the Scale of Months and Days expressed on the under Plate, the Axis of the *Terrella* is kept all the while parallel to itself, by means of the second Wheel placed between the two above-mentioned; and so the Change of the Sun's Declination, or rather, which comes to the same Purpose, the different Position of the Equatorial Axis with respect to the Circle separating Light and Darkness, is exhibited all the while the Earth is going round in her Orbit. By placing the Axis of an ivory Ball having one half blacked, upright in the middle of the Circle which carries the *Terrella*, this little Instrument will serve to explain the *Phænomena* of the Moon's *Phases*.

Having thus learned the Cause of the Sun's Change of Declination, we may now have recourse to the larger Globe, and moving it according to the different Seasons, we may observe the *Phænomena* thence arising more distinctly.

For a graduated Meridian, I had a flexible Slip of Brass divided into Degrees, which I could fix occasionally in the two Hour Circles; and upon such another Slip I had a Scale of Months, answering to the Sun's Declination, reckoning both ways from the Equator. By means of this graduated Meridian, the Globe being rectified according to the Sun's Declination, if we gently turn it round it's Axis, we may presently find the Time of the Sun's rising or setting in all Places, by observing the Hour Circle, when the several Degrees of Latitudes respectively come to the Horizon.

After the same manner, if the Globe be elevated to any particular Latitude, and the Meridian having the Scale of Months be fixed in it's Place, we may soon find the Time of the Sun's rising or setting in that Latitude throughout the Year, by observing the Hour Circle when the respective Days come to the Horizon. This Method is not only useful on the Account of it's being expeditious, but also because it intimates, why at the same time the Days are of different Lengths in different Latitudes, and in the same Latitude at different Times of the Year.

The Globe-makers might save us the Trouble and Expence of having these graduated Slips of Brass, by dividing some Meridian, which goes over the least Land, into Degrees, which might be marked with round Dots, and every Tenth numbered. The Scale of Months might be engraven upon some other Meridian. It would be of Use likewise, if the Parallels and Meridians of every Degree between the Tropics be drawn in

in faint Lines, which I think might be done without obſcuring the Map.

Parallel to the Horizon, and 18° below it, I had a Circle fixed for ſhewing the Limits of the Twilights: This is uſeful, as it ſhews at one View the State of the Twilights, and alſo why they do not lengthen or ſhorten, as the Days do. The Semi-Circle of Poſition is a thin narrow Plate of Braſs as uſual, but made ſo that it's Axis is moveable quite round the Horizon. I had alſo a narrow flexible Slip of Braſs, which might be girt round the Globe in any Poſition, and ſo be made to repreſent any great Circle whatſoever: This occaſional Circle may be inſtructive to Beginners on ſeveral Occaſions.

If the principal Horizon be of Wood, or made ſo as to obſcure the Globe below it, the Twilight Horizon had beſt have ſmall Feet of a proper Length, fixed ſo that it might ſtand in it's proper Place upon the other, occaſionally; then inverting the Poſition of the Globe, the ſame thing will be ſhewed as before.

The farther Uſe and Application of theſe Contrivances to different Projections of the Sphere, *&c.* will be obvious to thoſe who are acquainted with theſe things; and without dwelling any longer upon this Subject, it may ſeem, that I have already ſaid more than was needful in this Place. But the Globe being in every body's Hands, and in reality a very uſeful, entertaining, and inſtructve Inſtrument, I thought an Attempt to render it more ſo, would not be altogether uſeleſs, or yet unworthy the Notice of the Curious.

The Conſtruction and Uſe of Spherical Maps, or ſuch as are delineated upon Portions of a Spherical Surface. By Mr John Colſon, M.A. *F. R. S.* No. 440. p. 204. Jan. *&c.* 1736.

VIII. Geographical Maps, and Hydrographical Charts, though they are Repreſentations of a Convex Spherical Surface, yet were firſt delineated upon Planes, as being the moſt eaſy and obvious, tho' not the moſt natural and accurate Repreſentations: And they will be ſufficiently near the Truth, when the Part of the Earth or Seas to be deſcribed is not of a very large Extent. Such as theſe have been uſually called Chorographical and Topographical Maps; but when the Map is any thing general, or is to contain any large Tract of the Earth or Seas, ſuppoſe (for Inſtance) one of the four Quarters of the World, as they are called; then, when they are projected, or repreſented upon a Plane, the Parts muſt neceſſarily be diſtorted, one way contracted beyond the Truth, another way dilated, ſo as to give no juſt Idea of the whole. Nor can this Diſtortion be poſſibly avoided, when any conſiderable Part of a Spherical Surface, by any Projection whatever, is to be repreſented upon a Plane. 'Tis true, this Diſtortion is always regular, and according to certain Laws; ſo that knowing the Nature of the Projection, it may tolerably well be allowed for. But to do this ſcientifically, and as it ought to be done, requires much Skill and Accuracy in the Maker, as well as good Proficience and Experience in the Peruſer; and therefore not ſo proper for an Introduction to Learners, in the Rudiments of Geography. Young Minds are apt to receive wrong Notions and Prejudices

judices from them, at leaſt cannot be rightly and eaſily inſtructed by them.

To obviate this Inconvenience, Geographers have contrived and conſtructed the Terreſtrial Globe, on which they endeavour to delineate all the Parts of the Earth's Surface in their natural State, as to Longitude, Latitude, Diſtance, Bearing, Magnitude, &c. which being a true and genuine Repreſentation of the whole Superficies of the Earth, as far as it is yet known, is the beſt adapted for conveying juſt Notions to young Minds, and for preventing all falſe Conceptions and Prepoſſeſſions. After the firſt Rudiments of Geography have been imbibed from hence, they will be then prepared for the Uſe of plain Maps; and they will afterwards find, that large Projections of particular Countries, Kingdoms, and Provinces, *in plano*; will be of excellent Service to them for their farther Improvement in this uſeful and neceſſary Science. Nor will they now be in any Danger of being miſled by ſuch Maps, tho' they are not ſo juſt and natural Repreſentations of the Earthly Globe.

Now the ſame Conveniencies that may be derived from the whole Globe, may, in Proportion, be had from any notable Portions of it; as an Hemiſphere, a Quadrant, a Sectant, an Octant, or other Part. But with this Advantage beſides, that theſe partial Spherical Maps will not only be much leſs cumberſome, and more manageable than a whole Globe, but may be made much more accurate and particular, as being capable of being formed to a much larger Diameter than a Globe can conveniently be made to. The Maps may firſt be printed upon a Plane, as is uſual in the common Globes, and then paſted upon thin convex Shells of Paſteboard, formed to the intended Radius. The forming of theſe ſpherical Coats of Paſteboard will be a Matter of no great Difficulty, even to as large a Diameter as ſhall be deſired; but the chief Art will be required in projecting the Maps *in plano*, after the ſimpleſt and exacteſt Manner, ſo as that they may adapt themſelves, with as little Error as poſſible, to a ſpherical Surface. For a plane Surface cannot be converted into a ſpherical Surface without ſome Error. The beſt Method of doing this, with the leaſt poſſible Error, I think will be as follows.

Inſtead of the uſual Slips or Guſſets, as is the manner of Globe-makers, which are comprehended between two Meridians at ſome Diſtance, and are formed only tentatively and mechanically, without the Help of any juſt Theory, we may divide the whole ſpherical Surface into parallel Portions, or Zones; that is, into Parts terminated by two Parallels to the Equator, at the Diſtance (ſuppoſe) of ten Degrees. As if the firſt of theſe Portions, or Zones, were at the Equator itſelf, and extended to 5° of Latitude on each Side of that Circle, the ſecond Zone would be at the Parallel of 10° of Latitude, and would extend to 5° of Latitude on one Side, and to 15° of Latitude on the other Side of that Parallel, and ſo of the ſucceeding Zones.

Now we may conceive the firſt of theſe Portions, or Zones, to be converted from a ſpherical Surface to a plane Surface in this manner,

without ſenſible Error. Let the middle Line of this Zone, that is the Equator, continue in it's Situation, and let the Segments of the Meridians on each Side be conceived to unbend themſelves gradually, 'till they are extended into right Lines perpendicular to the Equator: Then will that which was before a Zone, or Portion of a ſpherical Surface, with a ſmall Alteration become a Portion of a cylindrical Surface, circumſcribed about the Sphere; whoſe Breadth is every where equal to 10° of the Sphere, and whoſe Circumference is equal to the Equator. And thus every Parallel to the Equator, as far as that of 5° of Latitude on each Side, will be ſtretched and extended into a Circle as large as the Equator; but they will all keep the ſame Diſtance from one another, and from the Equator, that they had before. This Extenſion, or Alteration, will be every where regular and uniform, and will be but very little, even where it is moſt: For the leaſt of theſe Circles, which is the Parallel of 5° of Latitude, has the ſame Proportion to the Circle it is ſtretched to, or the Equator, as the Sine of 85° has to the Radius, or as 9961947 to 10000000; which approaches very near to a Ratio of Equality. And now it will be eaſily conceived, that without undergoing any other Alteration, or Diſtortion, this Portion of a cylindrical Surface may be rectified, or extended into a plane Parallellogram, whoſe Length will be equal to that of the Equator, and whoſe Breadth will be equal to an Arch of 10° of the ſame Equator.

And conſequently, by an Operation that will be juſt the Reverſe of this, if upon a Plane we delineate ſuch a Parallellogram as this, we may then lay down all the Places that are contained in it very exactly, in their proper Situation of Longitude and Latitude; and then apply it's middle Line, or Equator, to that of a Globe of a due Magnitude, which will then become a Portion of a cylindrical Surface, circumſcribed about the Globe. Then by preſſing it cloſe to the Body of the Globe, we ſhall cauſe it to contract itſelf a very little, but regularly, which Contraction will be only according to Longitude, and not at all according to Latitude; and then the cylindrical Surface will be changed into that of a Sphere, and will become the firſt ſpherical Zone before deſcribed, with all it's Delineations in their due Poſition, without ſenſible Error.

In like manner in the ſecond ſpherical Portion, or Zone, comprehended between the Parallels of five and fifteen Degrees, whoſe middle Line is the Parallel of ten Degrees, we may conceive the Segments of the Meridians to unbend gradually on each Side, and to extend themſelves into Tangent right Lines, which therefore will form a Segment of a conical Surface, ſtill touching the Globe in the Parallel of ten Degrees of Latitude. The Axis of this Cone will coincide with the prolonged Axis of the Globe, and the Side of the Cone, which is to be eſtimated from the Vertex to the Circle of Contact, will be the Co-tangent of the Latitude, or the Tangent of 80°. Now this Portion of a conical Surface may eaſily be conceived to be unrolled, or to be expanded into a plane Surface, without undergoing any other Alteration, and then it will be-

come a Portion of a Sector of a Circle; which Portion will have for it's Length, or middle Line, an Arch of a Circle described with the aforesaid Tangent, as a Radius, whose Length will be the same as the Parallel of Contact, and it's Breadth will be equal to an Arch of the Equator of 10° as before. This Segment of a Sector of a Circle so produced, may therefore be easily described *in plano*, and within it may be inserted all the Places belonging to it, according to their Longitude and Latitude. Then it must be applied to the Globe, so as that it's middle Line shall coincide with the Parallel of 10°; then by pressing it may be bent to the Surface of the Globe, every Meridian to it's respective Representative, by which it will uniformly contract a little according to Longitude, but not at all according to Latitude. And thus the Globe will be covered as far as 15° of Latitude.

The next Zone, or that belonging to the Parallel of 20°, may be thus constructed *à priori*. Upon a plain Paper, with Radius equal to the Tangent of 70 Degrees, describe an Arch, whose Length is equal to that of the Parallel of 20°; as also two other concentrick Arches on each Side, at a Distance from the middle Arch equal to an Arch of 5°. This will be the required Segment of the circular Sector, in which are to be inserted all the Places belonging to it, according to their Longitude and Latitude. Then the middle Line or Arch is to be applyed to the Parallel of 20° upon the Globe, and the Segment of the conical Surface thence arising is to be duly contracted as before, or pressed close to the Globe; by which Means this Zone will also be compleated. And in the same manner we are to proceed to the succeeding Zones, 'till the whole Globe is covered. And the Method will not differ in any material Circumstance, if instead of a whole Globe, we are to construct any Part of it only, or what I here call a Spherical Map.

To reduce this Theory to Practice, and as a Specimen of Spherical Maps, I have constructed a Terrestrial Hemisphere to a Diameter of near 15 Inches; To which I have given the Name of the *British Hemisphere*, because it has *Great-Britain* in the Centre, or rather at it's Vertex. It is therefore adapted to the Meridian and Horizon of *London*, and exhibits one half of the Earth's Surface, as it lies round about this City; which is vastly the most considerable Part of the whole Earth's Superficies. The Longitude and Latitude of Places are here easily known by Inspection, and their Bearing and Distances may be nearly estimated: And all the Delineations are as accurate and particular as this small Radius would permit. I conceive therefore it may be no unfit Instrument for instructing Beginners, or for initiating young Minds in the first Rudiments of Geography.

A Spirit Level to be fixed to a Quadrant for taking a Meridional Altitude at Sea.

IX. The Necessity of seeing the Horizon, in order to find the Latitude of a Ship at Sea, has always been so great an Inconvenience, that any Method for determining it without the Help of the Horizon, will be of considerable Use, although it should be liable to an Error of a few Minutes: And as it is generally agreed by Seamen, that they are much

when the Horizon is not visible. By John Hadley, *Esq; V. Pr. R.S.* No. 430. p. 167. Nov. *&c.* 1733. Fig. 109.

oftner ſenſible of this Inconvenience in calm Weather, than in rough; it is hoped that the following manner of conſtructing and uſing a Spirit Level, may, in that Caſe, be capable of ſo much Exactneſs, at leaſt, as may render it acceptable to the Publick.

This Level is compoſed of a Glaſs Tube A B, bent into an Arch of a Circle, and containing ſuch Number of Degrees as will be moſt ſuitable to the Degree of Exactneſs with which the Obſervation can be made. The Bore of it muſt not be wider than $\frac{1}{10}$ of an Inch in Diameter, that the Liquor in it may the better keep together, and the two Ends of it ſtand Perpendicular to the Tube in all Poſtures: Nor ſhould it be much leſs, leaſt the hanging of the Spirit to the Sides hinder it from ſettling ſo truly by it's Weight to the loweſt Part of the Tube. This Tube is cemented into another Braſs one C D E F, of the ſame Curvature, the outer Half of which is taken off, ſo as to ſhew the Glaſs, leaving only a ſmall Part in the Middle D F entire, in which a ſmall Stop-cock G is placed. The Glaſs Tube is divided in two in the Middle, to make room for this Stop-cock, the Key of which muſt be pierced through with a Hole of only about $\frac{1}{100}$ Part of an Inch, for the Paſſage of the Liquor. The outer Ends of the Glaſs Tube muſt have a Communication with one another round about by Means of two ſmall Pipes I and K, and the Tube H, the manner of which is ſufficiently ſhewn by the Figure.

Each half of the Glaſs Tube A B muſt have a Scale of Degrees anſwering the Curvature of the Tube, ſubdivided at Pleaſure. They may be numbered either as the upper or under Scale in the Figure; and obſerve that in the under Scale two Degrees are numbered as one; the Reaſon of which is, that the Motion of the Spirit in the Tube increaſing the Number on one Hand, and at the ſame Time as much diminiſhing that on the other, their Difference is altered thereby, ſo as to anſwer to double that Motion. The Diviſion of the Scales are cut on the Edge of the Braſs half Tube, or Trough, which is made thick for the greater Strength.

In one of the ſmall Pipes I or K, juſt againſt the Return of it, which enters the End of the firſt-mentioned Glaſs Tube at A or B, is a ſmall Hole, by which to introduce into it ſo much Spirit of Wine as may fill it from the Middle of the Scale on one Hand to the Middle of that on the other; this Hole may be afterwards ſtopped by a Screw-pin.

The inner Ends of the two Halves of the Glaſs Tube A B ſhould be fixed into the entire Part of the Braſs Tube D F with a Cement made with old hard Bees-Wax, or ſome other Materials not diſſolvable by Spirit of Wine, as ſhould alſo the Ends of the ſmall Pipes I and K into this and the Tube H: Thoſe Halves, as to the remaining Part of their Lengths, may be faſtned down with any ſtrong Cement.

This Level may be ſet on to one of the Limbs of the Quadrant, fitted up for this Purpoſe, in the manner expreſſed in the Figure. It hath an Index moveable on the Centre, and a Spring at the other End to keep it ſteady, when it is directed to any of the Diviſions on the Arch, which

needs

needs no other Divifion than into whole Degrees. The Index may be furnifhed either with plain Sights, or may carry a fhort Telefcope, with a Vane in it's Focus, to receive the Image of the Sun, when it is bright enough; but if the Sun be hazy, or the Moon, or a Star be obferved, a fliding Shutter may be drawn out to tranfmit the Rays of Light to the Eye-glafs. The Vane has alfo a Thread fixed on it perpendicular to the Plane of the Quadrant. The whole Inftrument (for the eafier managing it) may be fupported by a Staff, refting with one End on the Floor.

The manner of ufing it is thus: Holding the Quadrant in a vertical Pofture, with that Limb to which the Level is fixed parallel to the Horizon, raife the Index to fome Divifion of the Arch, as near as you can to the true Height of the Object; which is fuppofed to be near the Meridian, and confequently to alter it's Altitude but fhowly; Then turning the Key of the Stop-cock, fo as to let the Spirit of Wine pafs through the fmall Hole in it, keep the Image of the Object as clofe to the Thread on the Vane as you can, endeavouring that the unavoidable Vibrations of it above and below the Thread, may be equal, both in refpect of their Length, and the Swiftnefs of their Motions, *&c.* Continue this 'till the Spirit feems quite fettled to fome Part of the Scale, and fomething longer. This it will do flowly, but without any fenfible Vibrations; for the Stop-cock allowing it no Paffage but through the fmall Hole in it's Key, will give fuch a Check to it's Motions, as not only to ftop thofe Vibrations, but alfo to hinder it's being thrown backwards and forwards in the Tube by any Shocks of the Inftrument; and yet as far as I have obferved will not prevent it's fettling (with fufficient Truth, though flowly) to the loweft Part of the Tube. About half a Minute of Time or more may be neceffary for this, according as the aforefaid fmall Hole is greater, or lefs in Proportion to the Bore of the Tube. When you judge the Spirit quite fettled, turn the Stop-cock again: It is of no Importance that the Image of the Object be exactly on the Thread at the Inftant that this is done. Obferve againft what Degree, and Part of a Degree, each End of the Spirit in the Tube ftands. If your Scale be numbered like the upper one in the Figure, and the Quantity of Spirit be exact, both Ends will agree, and the Degree and Parts marked muft be added to, or fubftracted from the Altitude fhewn by the Index, according to the Directions: If the Ends do not exactly agree, take the Mean between them. If you ufe the under Scale, fubftract the lefs Number from the greater, and add, or fubftract the Excefs, the Number refulting will fhew the mean Elevation of the Index during the latter Part of the Obfervation, and will differ from the true Altitude of the Object about half fo much as the Vibrations of it's Image above and below the aforementioned Thread on the Vane fail of compenfating one another during that Time. If either End of the Spirit leave the Scale, the Index muft be removed three or four Degrees, and the Obfervation repeated.

Inftead of the Curve Tubes A and B, two ftrait ones might be ufed, fet together fo as to make a very obtufe Angle in the Middle; but then

it

it will be convenient to have the Quantity of Spirit more exactly fitted to the Scale, because the allowing for the Difference will be something more troublesome.

If the Observer have an Assistant to attend to the Level, while he himself observes the Object, the whole Apparatus of the Brass Tube, and Stop-cock, may be omitted, substituting in it's room only a Plug with a small Hole in it, which may be wrapped round with a very thin Slice of Cork, and so thrust down into the middle of the Glass Tube. The cutting the Glass Tube in half in the Middle may likewise be avoided, if instead of the Stop-cock at G, there be one fixed in one or both of the Pipes I and K, to open and stop the Passage of the Air, having a larger Hole in their Keys, there being also a Plug with a small Hole, thrust down into the Middle of the Tube, as before.

The Bore of the small Pipes I and K, and the Tube H, must not be so narrow as to make it difficult to reduce the Spirit into it's Place, if by any Accident either End of it should get into them.

I have been informed, that an Object may by kept in View without much Difficulty, even in pretty rough Weather, thro' a Telescope magnifying about ten times. Now as such Telescopes seldom comprehend an Area of much more than 1° in Diameter, or at most $1^{\circ}\ 20'$ it follows that the Axis of the Telescope is always kept within $40'$ at most of the Object, and that is the greatest Vibration of the Image above and below the Thread on the Vane. If this be allowed, it seems reasonable to expect that the Medium of the Vibrations one Way should not exceed the Medium of those the other, more than by about $\frac{1}{5}$th or $\frac{1}{6}$th Part of the greatest Vibration; *i. e.* about 7 or $8'$ the half of which will be the Error of the Observation. In still Weather it will probably be much less, if the Instrument be in the Hands of a Person moderately skilful in observing.

A Description of a Water-Level to be fixed to Davis's *Quadrant, whereby an Observation may be taken at Sea, in thick and hazy Weather, without seeing the Horizon; by* Charles Leigh, *Gent.* No. 451. p. 413. *dated* Nov. 3. 1737.

X. The Sea-Quadrant now in Use, called Captain *Davis*'s Quadrant, being invented by that ingenious Gentleman, for taking the Sun's Altitude, is an Instrument well known, universally approved, and sufficiently accurate; I say sufficiently, because it is well known to all Artists at Sea, that 5 or $10'$ Error (which is generally the most, if the Instrument be good, though the Motion be great) is a Trifle scarce worth the noting, either in sailing near a Meridian, or parallel Circle. This, together with a long Use of this Instrument, has, to my Knowledge, (having had the Experience of 17 Years in the Royal Navy) occasioned such a Fondness to it, that it would be no easy matter to dissuade the Navigator from the Use of it, to any other.

It is true, that when the natural Horizon is obscured by thick and hazy Weather, (which is very frequently the Case, especially off of our Chanel, the Banks of *Newfoundland*, &c.) this Instrument, as it now stands, is of no Use; which too often occasions melancholy Consequences, such as the Loss of Ships and Cargoes, and, what is still more valuable, our Seamens Lives. If therefore, to this Instrument, an Apparatus were added, such

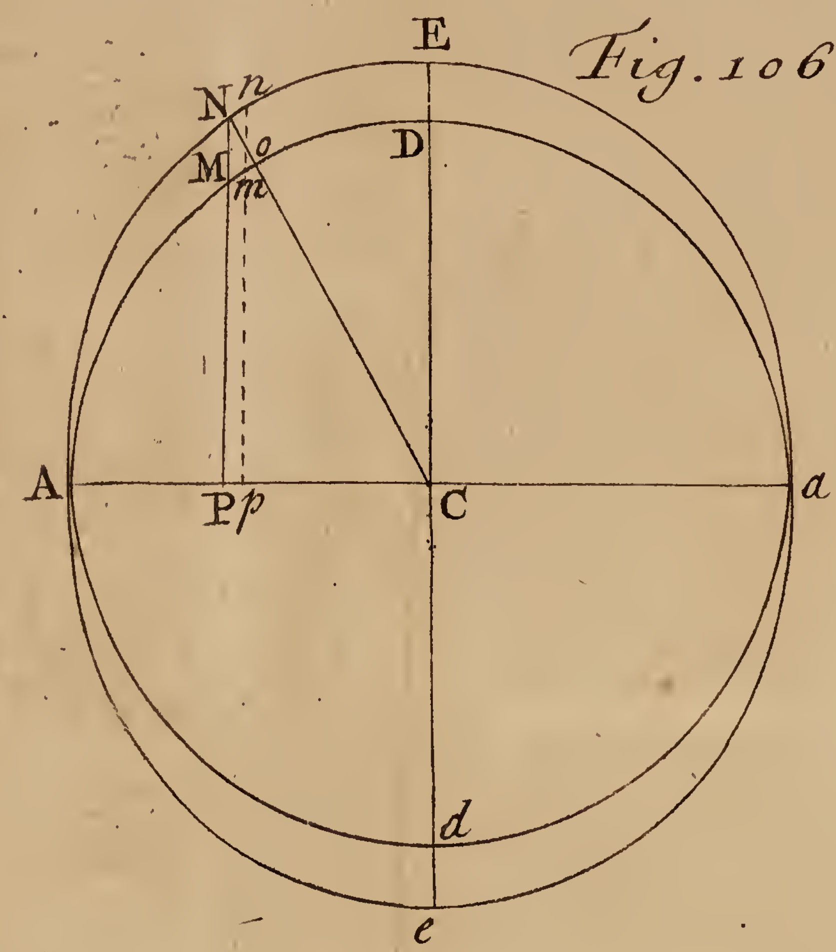

Fig. 106.

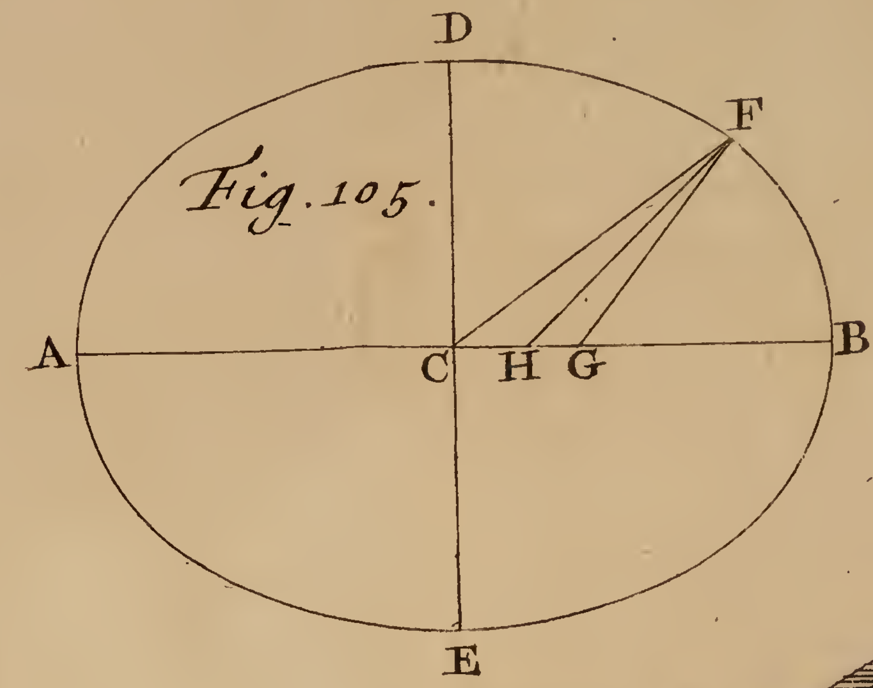

Fig. 105.

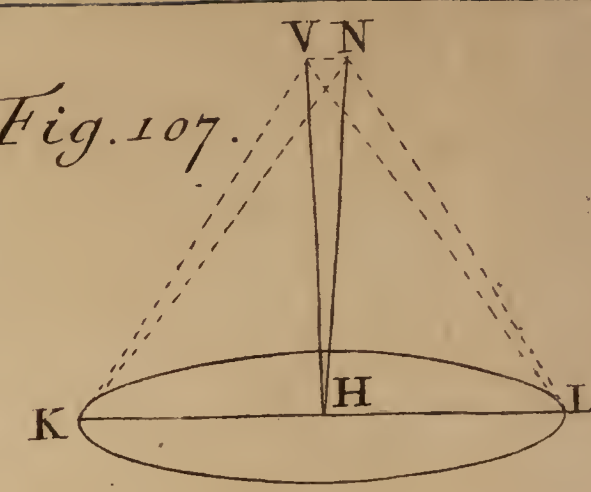

Fig. 107.

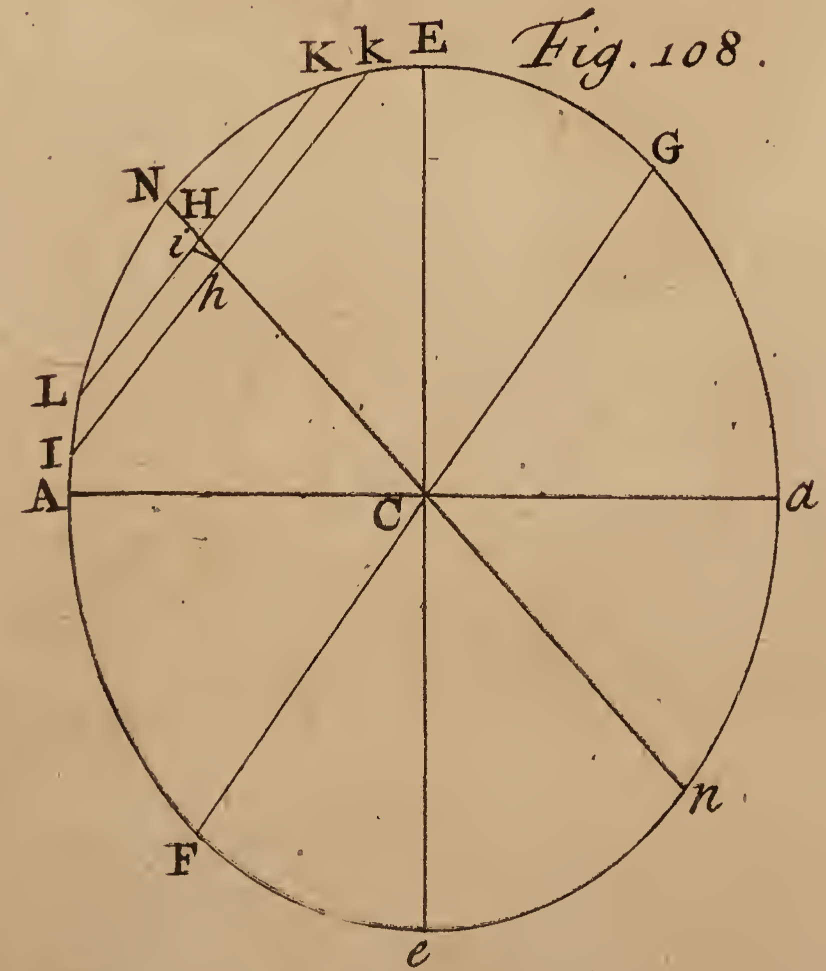

Fig. 108.

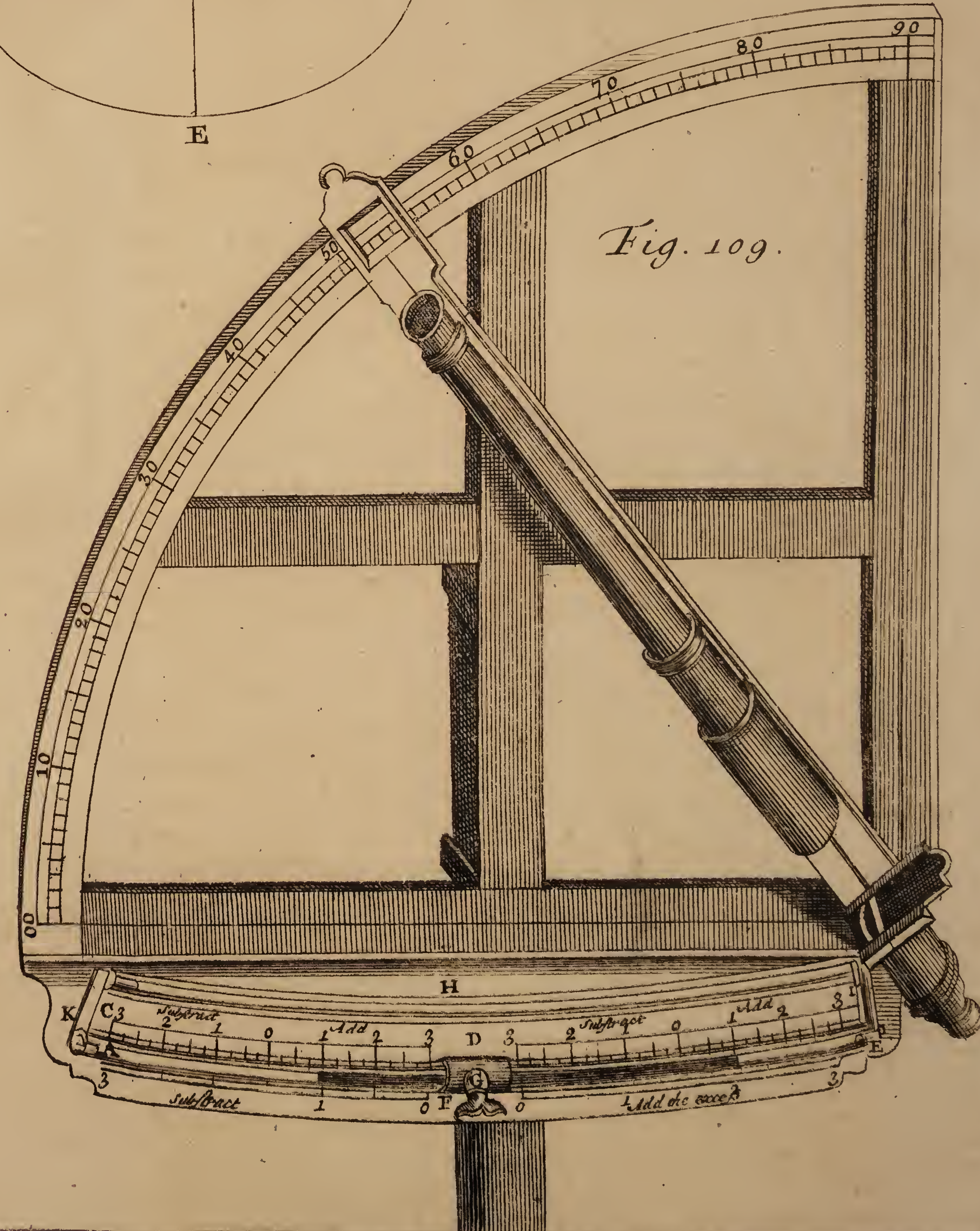

Fig. 109.

fuch as an artificial or portable Horizon, that could be as effectually relied on, as that of the true or natural; and at the fame Time plain, eafy, and obvious; I am of Opinion, it would be needlefs to go about proving it'sUfefulnefs.

To this End, fome ingenious Gentlemen have, within thefe few Years, very commendably employed their Talents this way; among which, I humbly offer my Mite.

I fhall now proceed to the Principle on which this Apparatus is founded, *viz.*

That the Surface of all Liquids (when free from any external Caufe) that have a Communication with each other, though divided and feparated in their Surfaces, will be truly in a horizontal Plain.

The Quadrant, and it's Conftruction, being well known, there remains but little to be faid to it; the principal Parts that I fhall take Notice of, are the two Sections of two different Circles that are concentrick, as A B, C D, on which the Degrees and Minutes are graduated; E, Fig. 110. the common Centre, through which goes a brafs Pin fixed to the Apparatus E F, which is an Index or *Radius* to the Section C D, on which Index is fixed a brafs Tube 15 Inches long, in the Extremities of which are fixed perpendicularly two Glafs Tubes E *h* and *d h*, 4 Inches long, with brafs Ferrels on the Tops.

On the central Pin, which is fixed in the Index, is alfo fixed the brafs horizontal Vane E *z* obliquely, in which there is a Hole for the central glafs Tube E *h*, to come through ¼ of it's Length, clofe to which, and from the common Centre, comes a white fine Thread, the End being fixed in the Vane E *z*; and in the fame manner is a Thread fixed clofe to the glafs Tube *d h*.

Directions to prepare, and obferve by this Inftrument.

To prepare this Inftrument for Obfervation, you muft pour Water (for that is always to be had) into the Tube E *h*, till it's little Surface rifes to the central Thread; then to keep it fixed there, fhut the Slide or Stop that is fixed on the Top of the central Tube, and there it will continue; then you may at Pleafure pour or drop Water into the Tube *d h*, till it's Surface alfo rifes to the Thread fixed there; and if too much Water is dropped in, dip in a Wire with a fmall bit of Spung or Cotton fixed to the End, till you exactly trim your Tubes; for in this lies the greateft Nicety and Exactnefs, to trim your Surfaces true to the Threads.

This being done, you are prepared for Obfervation; and placing yourfelf conveniently, where there is the leaft Motion, fit down on a Stool or the Deck, and having the Quadrant in it's proper Pofition on your Lap, open the Slide on the Top of the Tube E *h*, that the Water may have it's natural Tendency, which will be truly horizontal, conformable to the above Principle; then keeping your Eye on the central Thread, bring that and the little Surface into one, which will be effected

with the ſame Eaſe as if you obſerved by the natural Horizon; then keep moving the End of the Index F, till you bring the *Speculum* of the Sun in the little Hole on the Horizon-Vane that is cloſe to the Thread, ſo that you have, as it were, but one Object to look at during the Time of Obſervation: But if you uſe the Shadow-Vane, you muſt bring the upper Edge of the Shadow on the central Line, drawn on the Horizon-Vane, as uſual; remembering as often as you reſt, waiting the Sun's riſing, to cloſe the Slide, which prevents the Water's running out, it then remaining immoveable. And thus continuing to do, till the Sun is on your Meridian, caſt up the two Sums as is uſual, that is, the Degrees cut by the Shadow-Vane, and thoſe cut by the upper Edge of the Index on the greater Arch, which Sum will give what is required, *viz.* the Sun's Diſtance from the Zenith. On the End of the Index is fixed a Sight-Vane N, by which you may obſerve by the natural Horizon, the very ſame way as with the common Quadrant; ſo that the one will be the Proof of the other.

N. B. There are of late Invention, large Glaſs *Lens's*, very uſeful for collecting the weak and ſcattered Rays of the Sun into a *Speculum*; but if the Rays are even too weak to be collected by that, and that you have any Sight of the Sun, let another look through the little Hole on the Horizon-Vane above-mentioned, and the upper Edge of the Shade-Vane, to the Sun, and it will give what is required: The ſame Rule is to be obſerved in taking the Altitude of a Star.

The Deſcription and Uſe of an Apparatus added as an Improvement to Davis's *Quadrant, conſiſting of a Mercurial Level, for taking the Co-altitude of Sun or Star at Sea, without the uſual Aſſiſtance of the ſenſible Horizon, which frequently is obſcured. By the ſame Ibid. p. 417.*

XI. I had the Honour ſome time ago to communicate an Invention much upon the ſame Nature and Principle with this; ſince which I have made ſuch Alterations and Improvements thereto, as have rendered it complete and perfect for the Uſe intended, and have been confirmed by repeated Experiments, as well on board Ships, as on Shore. An Inſtrument of this Nature we greatly want at Sea, and it would be a great Satisfaction to me, if any Thoughts and Inventions of mine ſhould contribute to the removing of this grand Impediment, that ſo frequently happens.

To arrive to the utmoſt Perfection in Navigation, three things are abſolutely requiſite, *viz.* The Variation, the Latitude, and the Longitude; which laſt is, as yet, concealed from us. The two former indeed, we have a tolerable Certainty of, eſpecially the firſt which may be found by Obſervation, almoſt at any time the Sun ſhall be viſible in or above the Horizon, either by an Amplitude or Azimuth; but unhappily as yet, it is not ſo in regard to the Latitude, by any certain Method, but what is looked on as too abſtruſe for common Practice; for it is but once in 24 Hours that an Obſervation can be made from the Sun, and even that Space of Time ſo very ſhort, that if the Horizon ſhould then be obſcured, or a Cloud intercept the Rays of the Sun, the dead Reckoning

is then the only Guide, which, in Fact, is little better than groping in the Dark.

Since the Latitude then is our principal Guide at prefent, and liable to thefe Obftructions, it would be unneceffary to enlarge on the Advantages that would accrue to Navigation from Improvements tending to obviate them. As this Invention removes a very material Obftacle, *viz.* an obfcure Horizon, there remains another, which, I hope and believe, is not altogether impracticable to remove; and that is, being confined but to one fhort Space of Time for Obfervation, as already mentioned; and doubtlefs it would be of great Advantage to Navigation, could an accurate Method be found for difcovering the Latitude as frequently in the Day, as you may that of the Variation.

But to return to the Inftrument under Confideration, which is founded on this obvious Principle, *viz.* "That the Surfaces of all Liquids, "that have a Communication with each other, though feparated at any "Diftance in their Surfaces, will be in a true horizontal Plane."

The firft Inftrument that I made conformable to this Principle, was with a Water-Level; but finding that Water was fubject to fome Inconveniencies, I altered the Apparatus, and changed the Fluid from Water to Mercury: This Alteration and Improvement will more intelligibly appear by the Figure of the Inftrument, where A B, C D, reprefents Fig. 110. the Segments of two different Circles that are concentrick; E, the common Centre, in which moves the Pin or Axis fitted to the Index or Label E F; on which Label is alfo fixed the horizontal Tube G *g*, which has a Communication with the Two Glafs vertical Tubes E *h*, *d h*, in which moves the Mercury. On each Top of the vertical Tubes are fixed a large hollow brafs Cylinder *h h*, having in their Tops a Pin, by clofing of which, the included Air is prevented from any Communication with the External; by which means this Advantage is obtained, that it prevents, in a great meafure, that too quick and vibratory Motion, that is natural to the Fluidity joined to the Gravity of Mercury when moved, and at the fame Time, by having a fufficient Space and Quantity of Air in the Cylinders at Top, does not in the leaft impede the true Level; but notwithftanding this Precaution, the Mercury ftill would be fubject to a tremulous Motion, were it not that the Diameters of the vertical Tubes, to that of the horizontal, are as 2 to 1, and confequently the Area 4 to 1; by which means this Inconveniency is alfo removed, without any way affecting the horizontal Level.

The firft trimming or preparing the Tubes with Mercury is fufficient, and when the two little convex Surfaces of the Mercury appear juft vifible above the level Rings E *e*, then is the Inftrument correctly trimmed; if they appear much above or below the Rings, move the Tubes a little up or down, till the Surfaces are adjufted to the Rings; which is effected by means of the regulating Screw *l*, fixed at the End of the Bafe Tube.

As I well know the Fondneſs our Navigators have to *Davis*'s Quadrant, I adapted the Apparatus to this Inſtrument, which is ſo far from being perplexing, that it becomes obvious at firſt View, and by which an Obſervation can be made with great Facility; for the Obſerver may place himſelf in the moſt convenient Part of the Ship, where there is the leaſt Motion and Wind to diſturb him, and ſitting on a Stool or the Deck, holding the Inſtrument with his left Hand under the Horizon-Vane E *z*, and his Right at the End of the Label F, with his Thumb thereon, keeping the Label on the ſame Height or Level with his Eye, bring the left convex Surface of the Mercury to appear juſt viſible above the central Ring E, and the Shade or *Speculum* of the Sun from the Solar Vane *k*, to coincide therewith on the central Line E *z*; and the Sum of Degrees and Minutes cut on the two Arches by the Vane *k*, and the End of the Label F, will give, as uſual, the Angle of the Sun's Co-altitude. As the Sun riſes, the Shade will fall below the central Line (the Surface in it's proper Place); and when it paſſes the Meridian, and falls, it will appear above, ſo that the End of the Label muſt be moved in the ſame manner as the Sight-Vane uſually is.

To obſerve by a Star, another Perſon muſt look through the Slit on the Horizon-Vane, and over the upper Edge of the Shade-Vane, and bring the Star to coincide therewith, proceeding in the ſame manner as before, with the Sun.

There are two very oppoſite Cauſes of an obſcure Horizon; the one proceeds from thick hazy Weather, and the other from fine clear and calm Weather, as I have often experienced at Sea: I have been running with a Freſh of Wind, ſometimes five, ſix, and ſeven Days together, the Diſtance of 2 or 300 Leagues, without an Obſervation; and on the ſixth, ſeventh, or eighth Day, it has proved ſtark calm and clear Weather, but the Sea ſo ſmooth, and ſo like in Colour to the Sky, that the Edge or Circle of the ſenſible Horizon could not be diſtinguiſhed therefrom, and conſequently no Obſervation to be made by the Inſtruments then in Practice.

By this Improvement to *Davis*'s Quadrant, the above Obſtacles are entirely removed; ſo that an Obſervation can be made off of Headlands, in Harbours, on Shore, and, in ſhort, any where that a Sight of the Sun, *&c.* can be obtained, without any regard had to the Horizon; and, what is peculiar to it, is, that the true Level will be preſerved, as well on the Top of the higheſt Mountain, as cloſe to the Surface of the Horizon. The Apparatus is ſo contrived, that an Obſervation can be made with the ſenſible Horizon as uſual, by means of the Sight-Vane N, fixed near the End of the Label for that Purpoſe, ſo that the one will be a Proof to the other.

As the Succeſs of Inventions in all things of this kind muſt be confirmed by Experiments only, among many others, two were effectually made on board his Majeſty's Ship *the Oxford* at *Spithead*, in a high Wind when the Motion was ſhort and quick, and conſequently, a greater

Diſadvantage

Disadvantage than if on the high Sea, where the Motion is grave, slow and regular, occasioned by long Waves; but notwithstanding this quick Motion, the Observation made, exactly agreed with the Latitude of the Place; as will more evidently appear by the Report hereunto annexed, signed by all the Principal Officers that were then on board.

THE new Improvement made by Mr *Charles Leigh* to *Davis's* Quadrant, consisting of a Mercurial Level, for taking the Sun or Stars Altitude at Sea, when the sensible Horizon is obscured either by thick and hazy Weather, or in smooth Calms, when the Sky and Horizon are not distinguishable, was tried on board this Ship, when the Latitude by Observation made with the said Instrument agrees, as appears by the following Calculations; *viz.*

March the 9th, high Winds, and a quick Motion.	°	′		*March* 10th, ditto Weather.	°	′	
Sun's Zenith Dist. -	50	30		Zenith Distance - -	50	38	
Sun's Declination - -		15	S.	Declination - - - -		9	N.
Latitude by Observ.	50	45		Latitude by Observ.	50	47	

From which Experiment we judge this Instrument sufficiently accurate for discovering the Latitude, and removing that grand Impediment that frequently happens by an obscure Horizon, and consequently to be of great Use in Navigation.

From on board his Majesty's Ship Oxford, *at* Spithead,
March 10. 1738.

Signed,

Thomas Strachey, *First Lieutenant*.
Thomas Griffin, *Lieutenant*,
James Irving, *Master*.
William Slanning, *second Master*.

Note, The Latitude of *Spithead* the nearest about - - - - - - is } 50 46 North,

THE Alteration made in this Instrument is greatly for the better, for the Level of Water required to be trimmed every time of Observation, besides the Hazard of spilling the Water from a great Motion; *Directions concerning the Quadrant, &c.*
but

but in this Level of Mercury, the firft Trimming ferves always, and without hazard of fpilling, being clofe confined, as will be feen in the Inftrument. ——The Cylinders are made large enough to receive the Air that will be condenfed and rarefied alternately by the vibratory Motion of the Quickfilver through the fmall glafs Tubes, without affecting the true Level Line, as will be found upon Trial: Notwithftanding, the included Air has no Communication with the External, it's being clofe confined gives this Advantage, that it prevents the Mercury, in it's vibratory Motion, from being quick and tremulous.

The Bottoms of the brafs Cylinder that the glafs Tubes are fixed in, muft in the Infide be made Tunnel-wife, that the Mercury may not lodge behind. The Hole at the Top, and the Pin, is for taking out or putting in Mercury, if Occafion; as alfo to clean the Tubes with a Wire. The perpendicular Tubes muft at leaft be twice the Diameter of the long Bafe Tube, for this Reafon among others, that the dilating and condenfing of the Mercury, from Heat or Cold, may not be fenfible in the perpendicular Tubes; and alfo that the Bafe Tube muft be as long as the Index or Label will admit, and the Tube thereof to be as fmall as can be, but fo as to admit a Paffage for the Mercury. This Paffage fhould be through a fmall Glafs Tube inclofed in Wood, *&c.* The Cylinders muft not be foldered with foft Solder nor Silver: The Mercury will affect it.

Note, If the Mercury fhould be feparated by an Air-Bubble in the Tube, incline the Inftrument till the Mercury difappers in the Tube below the Bafe, and it will take it out. The true Level is when the little convex Surfaces of the Mercury juft appear above the Level Rings; then it is rightly trimmed; and when you obferve, you look only at one of them, *viz.* that at the Centre, the Shade-Vane co-inciding at the fame Time on the Horizon-Vane.

March 11. 1738.

An Account of Mr Thomas Godfrey's *Improvement of* Davis's *Quadrant, transferred to the Mariner's Bow, communicated to the* Royal Society, *by Mr* J. Logan. No 435. p. 441. *dated* Philadelpha June 28. 1734.

XII. Being informed that this Improvement, propofed by *Thomas Godfrey* of this Place, for obferving the Sun's Altitude at Sea, with more Eafe and Expedition than is practicable by the common Inftruments in ufe for that purpofe, was laft Winter laid before the *Royal Society*, in his own Defcription of it; and that fome Gentlemen wifhed to fee the Benefit intended by it more fully and clearly explained: I, who have here the Opportunity of knowing the Author's Thoughts on fuch Subjects, being perfwaded in my Judgment that if the Inftrument, as he propofes it, be brought into Practice, it will in many Cafes be of great Service to Navigation, have therefore thought it proper to draw up a more full Account of it, than the Author himfelf has given, with the Advantages attending it; which if approved of by better Judgments, to whom what I offer is entirely fubmitted, 'tis hoped the Ufe of it will be recommend-

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ed and further encouraged, as alſo the Author. The Riſe of the Improvement with it's Conveniencies, as alſo a Deſcription of it, are as follows.

Tho. Godfrey, having under the greateſt Diſadvantages (as I obſerved in my firſt Letter to Dr *Halley*, giving an Account of his Invention of the Reflecting Inſtrument) made himſelf Maſter of the Principles of Aſtronomy and Optics, as well as other Parts of Mathematical Science, applied his Thoughts to conſider the Inſtruments uſed in that moſt momentous Part of Buſineſs, Navigation. He ſaw that on the Knowledge of the Latitude and Longitude of the Place a Ship is in, the Lives of thouſands of uſeful Subjects, as well as valuable Cargoes, continually depend; that for finding the firſt of theſe, certain and eaſy Methods are furniſhed by Nature, if Obſervations be duly made: But *Davis*'s *Quadrant*, the Inſtrument generally uſed by Britiſh Navigators, (tho' ſeldom by Foreigners) he perceived was attended with this Inconveniency, that the Obſerver muſt bring the Shade or Spot of Light from the Sun, and the Rays from the Horizon, to coincide exactly on the fiducial Edge of the horizontal Vane; That tho' this can be done in moderate Weather and Seas with a clear Sky, and when the Sun is not too high, without any great Difficulty; yet in other Caſes it requires more Accuracy than can in ſome Junctures poſſibly be applied, and more Time than can be allowed for it. In *European* Latitudes, or to thoſe nearer the Northern Tropick, when the Sun is in the Southern Signs, and near the Meridian, he riſes and falls but ſlowly: Yet in Voyages to the *Eaſt* and *Weſt-Indies*, of which very many, eſpecially to the latter, are made, he is at Noon, often and for many Days together, in or near the Zenith, and when approaching to, or leaving it, he riſes and falls, when he has Declination faſter than even at the Horizon; for it is well known to Perſons acquainted with the Sphere, that when his diurnal Courſe takes the Zenith, he there riſes and falls a whole Degree or 60 Minutes, in the Space of $4'$ Time; ſo that the Obſerver has but $1'$, to come within $15'$ of the Truth in his Latitude: While in a middle Altitude, as 45° he is at Noon above $5'\frac{1}{2}$ in Time, in riſing or falling one ſingle Minute of Space, the Odds between which is more than 80 to 1. And yet, perhaps, no Parts of the World require more Exactneſs in taking the Latitude than is neceſſary in Voyages to the *Weſt-Indies*: For it is owing to the Difficulty of it, that Veſſels have ſo frequently miſſed the Iſland of *Barbadoes*, and when got to the Leeward of it have been obliged to run down a 1000 Miles further to *Jamaica*, from whence they can ſcarce work up again in the Space of many Weeks, againſt the conſtant Trade-Winds, and therefore generally decline to try for, or attempt it.

But farther, as the Latitude cannot be found by any other Method, that our Mariners are generally acquainted with, than by the Sun or a Star on the Meridian: In a cloudy Sky, when the Sun can but now and then be ſeen, and only between the Openings of the Clouds for very ſhort Intervals, which thoſe who uſe the Sea know frequently happens: As alſo in high tempeſtuous Seas, when tho' the Sun ſhould appear, the Obſerver

Obſerver can ſcarce by any Means hold his Feet; it would certainly be of vaſt Advantage to have an Inſtrument by which an Obſervation could alſo be, as it were, ſnatched or taken in much leſs Time, than is generally required in the Uſe of the common Quadrant.

Tho. Godfrey therefore conſidering this, applied himſelf to find out ſome Contrivance by which the Neceſſity of bringing the Rays from the Sun, and thoſe from the Horizon to coincide (which is the moſt difficult part of the Work) on one particular Point or Line from the Centre, might be removed. In order to which he conſidered, that by the 21. 3[d] *Elem. of Eucl.* all Angles at the Periphery of a Circle, ſubtended by the ſame Segment within it are equal, on whatever part of the Circumference the angular Point falls; and therefore, if inſtead of a Quadrant, a Semicircle were graduated into 90 Degrees only, accounting every two Degrees but one; this would effectually anſwer: For then, if an Arch of the ſame Circle were placed at the End of the Diameter of the Inſtrument, every Part of that oppoſite Arch would equally ſerve for taking the Coincidence of the Rays above-mentioned. But ſuch an Inſtrument would manifeſtly be attended with great Inconveniencies; for it would in great Altitudes be much more unmanageable, and the Vanes could not be framed to ſtand, as they always ought, perpendicular to the Rays. He therefore further reſolved to try whether a Curve could not be found to be placed at the Centre of a Quadrant, which would, at leaſt for a Length ſufficient to catch the Coincidence of the Rays, with Eaſe fully anſwer the Intention.

A Curve that in all the Parts of it would in Geometrical Strictneſs effect this, cannot be in Nature, any more than that one and the ſame Point can be found for a Centre to different Circles, which are not concentric. It is certain that every Arch on the Limb may have a Circle that will paſs through the Centre, and be a Locus or geometrical Place for the Angle made by that Arch to fall on: but then every Arch has a different Fig. 111. one from all others; as in the Figure. Let A B C be the Quadrant, and A B, E F, G H be taken as Arches of it: Circles drawn through each two of theſe reſpectively, and through the Centre C as a third Point, will manifeſtly be ſuch Loci or Places: For every Pair of theſe Points ſtand in a Segment of their own Circle, as well as on a Segment of the Quadrant; and therefore by the cited 21. 3[d] *Elem.* the Angles ſtanding on theſe firſt Segments will every where be equal at the Periphery of their reſpective Circles, and their Radius will always be equal to half the Secant of half the Arch on the Quadrant. For in the Circle C E D F (for Inſtance) the Angle C E D is right, becauſe 'tis in a Semicircle, C E is the Radius of the Quadrant, E D the Tangent of the Angle D C E $= \frac{1}{2}$ the Arch E F, and C D is the Secant of the ſame = the Diameter of the Circle CEDF, and therefore it's Radius is half that Secant.

Now from the Figure 'tis plain, that in very ſmall Arches the Radius of their circular Place will be half the Radius of the Quadrant, that is, putting this Radius = 10, the other will be 5. And the Radius for the

Arch of 90, the highest to be used on the Quadrant will be the Square Root of half the Square of the Radius = Sine of 45 Degrees = 7.071, and the Arches at the Centre drawn by these two *Radii* are the Extreams, the Medium of which is 6.0355. And if a circular Arch be drawn with this Radius $\frac{1}{20}$th Part of the Length of it, that is, in an Instrument of 20 Inches Radius, the Length of one Inch on each Side of the Centre affording 2 Inches in the whole, to catch the Coincidence of the Rays on, which must be owned is abundantly sufficient, the Error at the greatest Variation of the Arches, and at the Extremity of these 2 Inches, will not much exceed $1'$.

But in fixing the Curvature or Radius of this Central Arch, something farther than a Medium between the Extreams in the Radius is to be considered: For in small Arches the Variation is very small, but in greater it equally increases, as in the Figure, where it appears the Difference between the Angles A B C and A D C is much greater than the Difference between E B C and E D C, though both are subtended by the same Line B D: for their Differences are the Angles B A D and B E D. Therefore this Inequality was likewise to be considered; and compounding both together, *Tho. Godfrey* pitched on the Ratio of 7 to 11, for the Radius of the Curve to the Radius of the Instrument, which is 6.3636 to 10. But on further Advisement he now concludes on 6 $\frac{4}{10}$; and a Curve of this Radius of an Inch on each Side of the Centre to an Instrument of 20 Inches Radius or of $\frac{1}{20}$th of the Radius, whatever it be, will in no Case whatever, as he has himself carefully computed it, produce an Error of above $57''$; and 'tis very well known that Navigators (as they very safely may) in their Voyages entirely slight a Difference of one Minute in Latitude. Fig. 112.

This Radius is the true one for the circular Place to an Arch of $77^\circ\ 15'$, and the Variation from it is nearly as great at 90 Degrees as at any Arch below it, the greatest below being at about 44°, which is owing to the Differences expressed by the last Figure above, and not to those of the Curvatures or circular Places. Yet this Variation of $57''$ arises only when the Spot or Coincidence falls at the Extremity of the horizontal Sight or Vane, or a whole Inch (in an Instrument of 20 Inches Radius) from the Center, and then only in the Altitudes or Arches of about 44 or 90°. And in these, at the Distance of $\frac{1}{2}$ an Inch from the Center, the Variation is but $\frac{1}{4}$ so much, *viz.* about $14''$; and at $\frac{1}{4}$ of an Inch, not $4''$; at the Center 'tis precisely true. Therefore as an Observation may be taken with it in $\frac{1}{4}$ of the Time, that *Davis*'s Quadrant, on which three Things must be brought to meet, in a general way requires: I say, considering this, and the vast Importance of such Dispatch, in the Case of great Altitudes, or of tempestuous Seas, or beclouded Skies, 'tis presumed the Instrument thus made will be judged preferable to all others of the kind yet known. Some Masters of Vessels, who sail from hence to the *West-Indies*, have got of them made as well as they can be done here; and have found

ſo great an Advantage in the Facility and in the ready Uſe of them, in thoſe Southerly Latitudes, that they reject all others. And it can ſcarce be doubted, but when the Inſtrument becomes more generally known, it may, upon the *Royal Society*'s Approbation, if the Thing appear worthy of it, more univerſally obtain in Practice.

'Tis now 4 Years ſince *Tho. Godfrey* hit on this Improvement; for his Account of it, laid before the Society laſt Winter in which he mentions two Years, was written in 1732. And in the ſame Year, 1730, after he was ſatisfied in this, he applied himſelf to think of the other, *viz.* the reflecting Inſtrument by Speculums, for a help in the Caſe of Longitude, though 'tis alſo uſeful in taking Altitudes, and one of theſe, as has been abundantly proved by the Maker, and thoſe who had it with them, was taken to Sea and there uſed in obſerving the Latitude, the Winter of that Year, and brought back again hither before the End of *February*, $17\frac{30}{31}$, and was in my keeping for ſome Months immediately after. It was unhappy indeed, that having it in my Power, ſeeing he had no Acquaintance nor Knowledge of Perſons there, that I tranſmitted not an Account of it ſooner: But I had other Affairs of more Importance to me: And it was owing to an Accident which gave me ſome Uneaſineſs, *viz.* his attempting to publiſh ſome Account of it in Print here, that I did it at that Time, *viz.* in *May* 1732, when I tranſmitted it to Dr *Halley*; to whom I made not the leaſt Doubt but the Invention would appear entirely New. This, on my part, was all the Merit I had to claim, nor did I then, or now aſſume any other, in either of theſe Inſtruments. I only wiſh that the ingenious Inventor himſelf might by ſome means be taken Notice of, in a Manner that might be of real Advntage to him.

There needs not, I ſuppoſe, much more of a Deſcription of the Inſtrument than has been given: I ſhall only ſay, the Bow had beſt be an Arch of about 100 Degrees, well graduated, and numbered both ways; the Radius of 20 or 24 Inches; the Curve at the Centre to be $\frac{1}{20}$th of the Radius on each Side, that is, $\frac{1}{10}$th of it in the whole; the Radius of that Curve $\frac{64}{100}$ Parts of the Radius of the Inſtrument; that the Glaſs for the Solar Vane ſhould not be leſs, but rather larger, than a ſilver Shilling, with it's Vertex moſt exactly ſet. And that the utmoſt Care be taken to place the Middle of the Curve at the Centre exactly perpendicular to the Line or Radius of 45 Degrees. As the Obſerver muſt alſo take Care that the two Vanes on the Limb be kept nearly equi-diſtant from that Degree; to which I ſhall only add, that it may be beſt to give the horizontal Vane only one Aperture, and not two. The reſt I ſuppoſe may be left to the Workmen.

Fig. 113. *Note*, That the Radius of the Quadrant being divided into 20 equal Parts, the Centre X of the Curvature of the Horizon-Vane (A B) muſt be 12 $\frac{8}{10}$ of thoſe Parts from the Centre (C) of the

the Quadrant. The Breadth (A B or *g b*) of that Vane should be $\frac{1}{10}$ of the whole Radius, that is, $\frac{1}{20}$ on each Side of the Centre (C).

The Description and Use of an Instrument for taking the Latitude of a Place at any time of the Day; by Mr Richard Graham, *F. R. S. Ibid.* p. 450.

XIII. The necessity of finding the Latitude a Ship is in, is too well known to be insisted on: Frequent Opportunities of observing the Latitude must consequently be of very great Advantage to Navigation. The Method usually practised, is by taking the Sun or Star's Meridian Altitude or Zenith Distance: In this Case, if the Sun does not shine but for some small Time only, before Noon and after, though it be clear all the rest of the Day, it is of no use for this Purpose. Mr *Fatio*, *F. R. S.* (in the Year 1728) proposed a Method for finding the Latitude, from two or more Observations of the Sun (or Stars) at any Time, the Distance of the said Observations in Time, being given by a Watch; but as his Method requires a vast Number of Computations, and a great deal of Skill in Spherical Trigonometry, it has very seldom been made use of, and never but by good Mathematicians. The Instrument here described will answer the same End, and has these Advantages; *viz.*

1st, It may be very easily understood by Seamen.
2dly, It immediately shews the Latitude of the Place.
3dly, It gives the Time of Day at Sea when no other Instrument can.
4thly, It may be made as large, and consequently as accurate as is desired.

A Description of the Instrument. Fig. 114.

A B C represents part of the Hemisphere of a large Globe (half the Globe, and the Part below the Tropick are cut off, that it may take up the less room). A C, half the Equator, divided into 12 Hours above, and 180 Degrees below, and subdivided into Minutes, as is likewise the lower Tropick D D. E E, a moveable graduated Meridian, turning on the Axis F F. G an Index to fix it (by the means of the Screw H) to any Hour. I *i* I, a circular Beam-Compass, the Centre I *i* to be fixed on the Meridian to any Degree and Minute of Declination, by the Method commonly called *Nonius*'s Divisions: *k* the Point for drawing Arches, which is likewise fixed to any Degree and Minute by the same Method. As the Meridian is at some Distance from the Globe, L is a piece of Brass to fix on the Meridian, marked with *Nonius*'s Divisions, with a Point reaching down to the Intersection of the Arches, by which means the Distance of the said Intersection from the Equator, or it's Latitude is found. The Degrees and Minutes may likewise be shewn by diagonal Lines.

The Use of the Instrument.

Prop. I. *From two Observations of the Height of the Sun, the Distance of the said Observations in Time, being given by a Watch, as likewise the Declination of the Sun; to find the Latitude of the Place, and Hour of the Day.*

 I. *When*

I. *When the Ship is at Rest, that is, at Anchor, or in a Calm, so as to have little or no progressive Motion.*

Case 1. Suppose the Sun in the Equator, on the Day of Observation: Fix the Centre of the Beam-Compass at 0 Degree (or at the Equator,) and move the Point *k* to the Zenith Distance, (the Complement of the Altitude, taken by the usual Instruments,) and from any Hour, as from C, describe an Arch of a Circle with the said Point, as *b c* (*Ex.* 1.) Suppose eight Hours after, by your Watch, you have another Observation; move the Meridian 8 Hours farther, to *d*, and fix it there; and with the Zenith Distance then observed, describe another Arch as *e f*, the Point where it cuts the former is the Place of Observation, and it's Distance taken on the Meridian from the Equator shews it's Latitude; and the Minutes reckoned on the Equator from the Meridian to C and *d* (the Times of Observation) shew what those Hours were.

Case 2. When the Sun has Declination: Fix the Centre of the Beam-Compass on the Meridian, to the proper Degree of Declination for the Day of Observation, and proceed as before.

Case 3. If the Observations are at a greater Distance than twelve Hours, but in the same Day: Make use of the Complement to twenty-four Hours of the Distance in Time, and take the Declination on the contrary, or lower Side of the Equator; and instead of the Zenith Distances, take the Nadir Distances or Altitudes increased by 90°.

Thus you will find the Latitude, and Time of each Observation from Midnight. In this Case the Beam-Compass must extend to more than 90°.

Case 4. If the Observations are more than a Day asunder; as for Instance a Day and 2 Hours (26 Hours): Place the Centre of the Beam-Compass 2 Hours farther than it was the Day before; but in different Declinations, according to the Table of Declination for the several Days.

Case 5. When the Observations are made by a Star: The Centre of the Beam-Compass must be set to the Declination of the Star; then proceed as before. To find the Hour in this Case, the right Ascension must be likewise given.

Scholium. The same Method may be useful at Land, when no Meridian Observation offers.

II. *The Ship in Motion.*

Case 1. Suppose the Sun in the Equator: The Distance between the two Observations 8 Hours, as before, and the Arch *a a a* (*Ex.* 2.) described by the Zenith Distance of the first Observation, from the Centre C; and the Angle *c a b*, 40 Degrees, is the Angle between the Ship's way, and the Azimuth of the Sun continued, (given by the Azimuth Compass) and that during the eight Hours, the Ship has made 1°, or 60′ from

a to

a to *b*, or from the Sun; then, as Radius is to the Cosine of *c a b* 40°, so is *a b* 60′ to *a c* 46′; add 46′ to the Zenith Distance C *a*; and with *k*, the Point of the Beam-Compass set at that Distance, describe the Arch *c b e*; then with the Zenith Distance of the last Observation, whose Centre is *d*, draw the Arch *f f*; the Point where it cuts the Arch *c b e*, is the Place where the Ship was last; and it's Distance taken on the Meridian from the Equator shews it's Latitude; the Minutes reckoned on the Equator from the Meridian to *d* (the Time of the last Observation) shew the Hour, or it's Distance from 12 o' Clock.

If the Ship had sailed from *a* to β or towards the Sun: The Cosine of the Angle β *a* γ, or of the Angle between the Ship's Way and the Sun, must be substracted from the Zenith Distance of the first Observation. Case 2.

N. B. Only the two Arches *c b e*, *f f*, are to be drawn on the Globe, the rest being added here, to shew the Reason of the Construction.

To find the Latitude of the first Place: From the Equator, with a pair of Compasses, take the Distance sailed 60′, and with one Foot in the Intersection of the Arches *b e*, *f f*, the Place found before, put the other in the Arch *a a a*, the Zenith Distance of the first Observation, and in this Instance, on the left Hand of the Azimuth of the Sun, this is the Place sought; and it's Distance taken on the Meridian from the Equator, shews the Latitude; and the Minutes reckoned on the Equator from the Meridian to C, the Time of the first Observation, shew the Hour. Case 3.

The Interval in Time or Degree between the two Places, shewn by the Index G, is the Difference of Longitude.

N. B. Those Observations are best, whose Arches cross each other almost at right Angles.

Prop. II. *The Zenith Distances of two Stars, observed at the same Time, their Declination, and right Ascension being known; to find the Latitude of the Place of Observation.*

Fix the Centre of the Beam-Compass to the Declination of either of the Stars, and with the Zenith Distance of that Star describe an Arch; move the Meridian as many Hours farther as is the Difference of right Ascension of the other Star; and fix the Centre of the Beam-Compass to the Declination of it; and with it's Zenith Distance cross the first Arch: The Intersection shews the Latitude of the Place of Observation; and also the Distance of the right Ascension of the Zenith from that of either of the Stars, by which means the Hour may be known.

If a Celeſtial Globe is made uſe of, then place the Centre of the Beam-Compaſs over the ſeveral Stars.

The Latitude and Hour being given, the Variation of the Compaſs is eaſily known.

N. B. In order to draw Arches on the Globe; rub ſome Black-Lead powdered on a Piece of Paper; lay the Side which is blacked next the Globe, where you imagine the Interſection of the Arches will be: Then draw them on the clean Side with the Point of the Beam-Compaſs, and they will appear on the Globe; and if the Globe is well varniſhed, they may be rubbed out with Bread, or waſhed out with Water.

As Altitudes at Sea are now readily taken, with great Exactneſs, by the Quadrant invented by *John Hadley*, Eſq; *V. P. R. S.* and as the ſaid Altitudes are the Principles on which the Operations above deſcribed are founded; the previous Uſe of that Quadrant cannot but be of the utmoſt Importance to thoſe who ſhall have Occaſion for this Inſtrument.

The Deſcription and Uſe of this Inſtrument was laid before the *Royal Society*, *Dec.* 9. 1731; but as I knew Mr *Reid* was contriving one for the ſame Purpoſe, I delayed making mine Publick. His Method not yet appearing in Print, I have thought proper to communicate my own (eſpecially as 'tis now improved) conceiving it may be of ſome Advantage to Navigation.

The Uſe of a new Azimuth-Compaſs for finding the Variation of the Compaſs or Magnetic Needle at Sea, with greater Eaſe and Exactneſs than by any ever yet contrived for that Purpoſe; by Captain Chriſtopher Middleton, *F. R. S.* No. 450. p. 395. Oct. &c. 1738.

XIV. To diſcover the Declination of the Magnetic Needle, or Variation of the Compaſs at Sea, with ſome tolerable Degree of Certainty and Exactneſs, is a thing of great Uſe and Importance in the Art of Navigation.

The Inſtruments and Methods hitherto uſed for this Purpoſe, (as we could eaſily demonſtrate, if it were needful) are ſubject to ſeveral Inconveniencies, Errors, and Defects; to remedy which, this new *Azimuth-Compaſs* was contrived, and has by Experience been found effectual. It would be needleſs to give a Deſcription to ſuch as have the Inſtrument before them, and we ſhall therefore only ſhew the Manner of uſing it, and that as briefly as may be, which take as follows:

1*ſt*, The Inſtrument muſt be rectified, or fitted for Obſervation, by turning it about till the four Cardinal Points, that are hung upon the Centre-Pin, agree with the four Cardinal Points on the Chart, at the Bottom of the Box: Then will the Needle, that ſhews the Magnetic Meridian, ſtand at no Degrees, and the Eaſt and Weſt Points at 90°, on the graduated Circle within the Box; and in this Situation it muſt be kept, as near as may be, during the whole Time of the Obſervation.

2*dly*, Let the Index of the Quadrant be placed to that Degree of the Arch, on the Rim of the Box, which the Obſerver judges to be nearly equal to the Height of the Sun or Star whoſe *Azimuth* is ſought; for by this means the Object will be more readily found.

3*dly*, Turn the Quadrant round towards the Sun or Star, till it appear upon the vertical Hair within the Teleſcope, to an Eye looking through the ſmall Hole or Sight; and then ſlide the Index a little upward or downward on the Arch, till the Object by this means be brought to coincide or touch the viſible Horizon.

Laſtly, The Degrees and Minutes then marked by the Index upon the Arch of the Quadrant, will ſhew the Altitude of the Object, which will always be the ſame, whether the Inſtrument is in Motion or at Reſt; at the ſame time the Degree cut by the Index on the horizontal Rim or Circumference of the Compaſs-Box, will give the magnetical *Azimuth* of the Sun or Star.

N. B. All this may be performed by one Perſon, whereas the old Compaſs requires ſeveral to manage it, which alſo makes it ſubject to many great Errors.

How the Variation of the Needle is found by means of Magnetical *Azimuth* and Altitude thus obtained, is taught in every Treatiſe of Navigation, and we have no need to repeat theſe Rules in this Place. But as the Reſolution of this Problem is ſomewhat troubleſome, and requires ſuch a Knowledge of the Doctrine of the Sphere, as every Seaman has not attained, we ſhall here exhibit an eaſy Method of diſcovering the Variation of the Compaſs without any manner of Calculation, which cannot fail to render this Inſtrument ſtill more acceptable: To this End,

1*ſt*, Let the Magnetic *Azimuth* of the Sun (or any Star, when it is near the prime Vertical, and conſiderably elevated above the Horizon) be found according to the Directions already given, before it arrive at the Meridian, and note well the Altitude, or let the Index remain fixed at ſame Point on the Arch.

2*dly*, Find the Magnetic *Azimuth* of the Sun or Star in like manner as before, when it is exactly at the ſame Degree of Altitude, after it has paſſed the Meridian: And,

3*dly*, If theſe two Magnetical *Azimuths* are equal, the Needle has no Variation: If unequal, add them together, and half their Sum will be the true *Azimuth*; or ſubtract the leſs from the greater, and half the Difference will be the Variation required. The Circumſtances of the Obſervation will the more readily diſcover whether the Declination is Eaſterly or Weſterly.

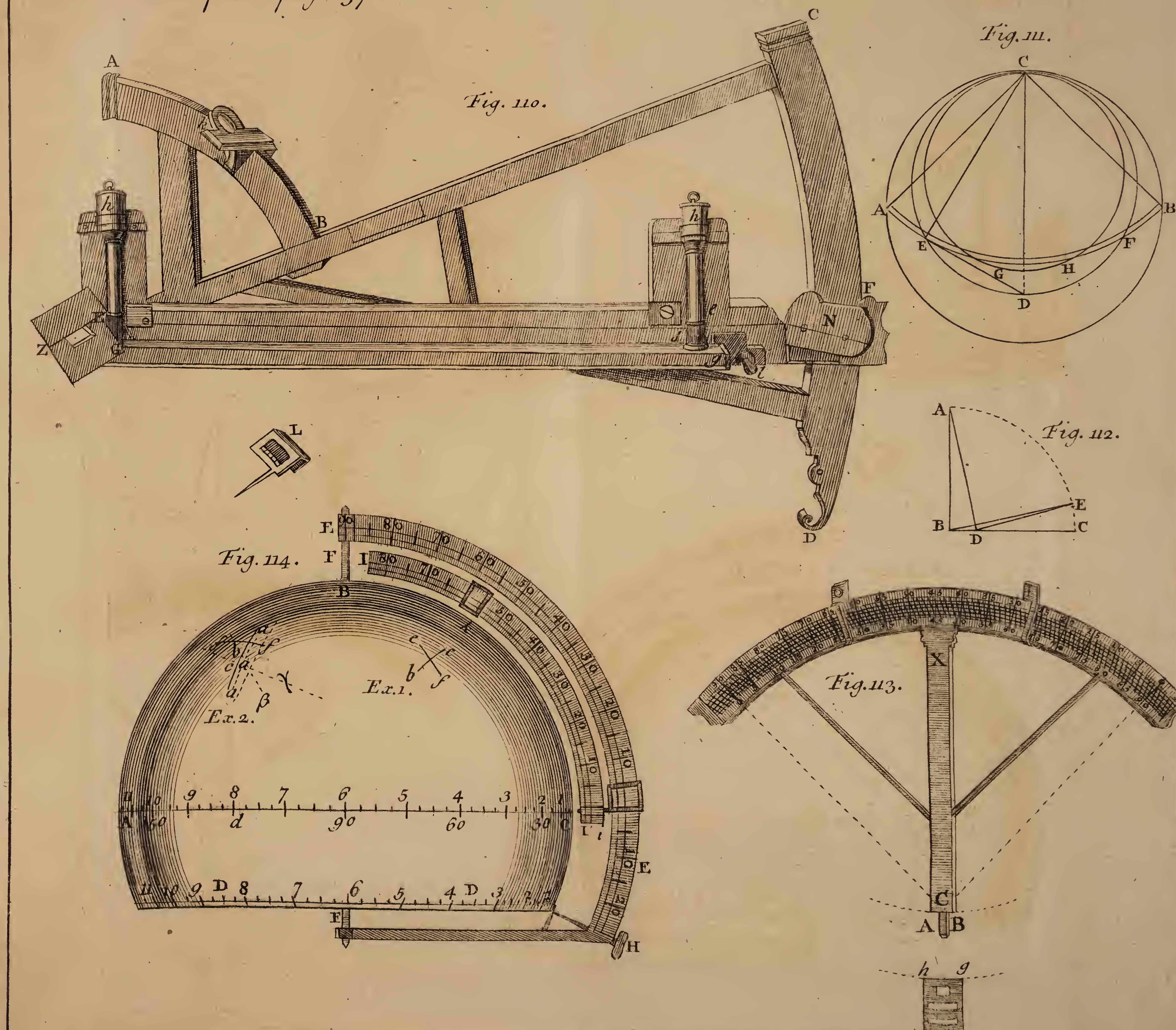
N. B.

N. B. Though it would be very commendable in Gentlemen who uſe the Sea, to learn the Names of moſt of the principal Fixed Stars, yet even that Knowledge is not neceſſary in the Uſe of this Inſtrument: Neither is it needful in this caſe to know exactly the Latitude of the Place of Obſervation, provided the Difference of Latitude between the Obſervations be not very great: It is ſufficient, that Care be taken to obſerve the ſelf-ſame Star, before it comes to the Meridian, and after it has paſſed it; and for the ſake of greater Exactneſs, the Caution before given ſhould be regarded, to wit, That the Star be at ſome conſiderable Height above the Horizon, and alſo near the prime Vertical.

XV. *(See the folded Sheet.)*

The End of the FIRST PART.

Pla. XX. Vol. VIII. part 1. page 376.



bſervations made of the Latitude, Variation of the Magnetic Needle, and Weather, by Capt. Chriſtopher Middleton, in a Voyage from London to Hudſon's-Bay, Anno 1735. No. 442. p. 270. July, &c. 1736.

Month.	Day.	Hour.	Altitude of the Thermometer. ° ′	Altitude of the Barometer. ° ′	Latitude by Account. ° ′	Longitude from *London* by Account. ° ′	Variat. of the Comp. W.	Latitude obſerved by *Smith's* Quadrant. ° ′	Latitude obſerved by *Hadley's* Quadrant. ° ′	Latitude obſerved by *Elton's* Quadrant. ° ′	Latitude by a Sextant of *Ward* and *Smith*. ° ′	Winds.	Remarks.
		6	27	29. 9									
ay	31	12	24	29. 6	60. 0	19.11	23					S W to S E	Variable, with ſome Rain.
		9	29. 7	29. 7									
		6	29. 7	29. 7									
ne	1	12	25	29. 5	59.50	23.33	24					S E to S W	Variable the firſt two Parts, moderate the latter, freſh Gales and a great Sea.
		9	25	29. 5									
		6	26	29. 4									
	2	12	26	29. 3	59.55	25.53	25	59.54	59.59			S W to S by E	Cloudy, and ſqually with Rain.
		9	27	29. 8									
		6	28	29. 9									
	3	12	26	28. 8	60	28. 3	26	59.57	60			S W	Frequent Showers of Rain, with Squalls.
		9	27. 9	30. 1									
		6	29	30. 5									
	4	12	26	29. 8	60.40	29.25	26	60.39	60.41			S W	Uncertain ſqually Weather, with a Weſtern Swell.
		9	27. 9	30. 1									
		6	28	30									
	5	12	26	29 8	60.31	32.12	27					S W	Squally, with ſome Rain.
		9	25	29. 7									
		6	28	29. 7									
	6	12	25	29. 5	58.44	35.46	27	58.47	58.50			S W to N N W	Variable, with Squalls of Rain.
		9	24	29. 5									
		6	28	29. 5									
	7	12	28	29 9	58.11	36.58	28	58. 8	58.11			N N W to W N W	Variable freſh Gales, with Rain and Squalls.
		9	30	29. 8									
		6	30	29 9									
	8	12	28	29. 8	57.50	38.33	28	57.53	57.57			N N W to W by S	Moderate and fair Weather, with Clouds.
		9	26	29. 6									
		6	26	29. 9									
	9	12	25	28. 9	58.28	42.16	28	58.28	58.38			W by S to S by E	Rain and freſh Gales.
		9	26	29. 6									
		6	31	29. 8									
	10	12	28	30	58.25	43.11	29	58.16	58.22			N W	Hard Gales, with a great Sea.
		9	26	30 4									
		6	30	29. 8									
	11	12	27	29. 8	58.48	44.16	29	58.40	58.45			N W to N E	Moderate, with ſome Rain.
		9	28	29. 9									
		6	30	30									
	12	12	28	29. 6	58.22	45.46	29					N N E to W by S	At 6 made Cape *Farewell*, from N b W to N b E 14 or 15 Leagues diſt. Moderate with ſome Rain.
		9	28	29. 8									
		6	27	29									
	13	12	27	29. 7	58.12	45.37	29	58.10	58 13			N W	Much Ice in Sight. An hard Gale, with a great Sea.
		9	32	29 9									
		6	31	30. 1									
	14	12	26	29. 9	58.12	46.11	30	58.10	58.11			N N W to E S E	Moſt part hard Gales. Under Main Sail and Mizen.
		9	29	30.23									
		6	29	30. 5									
	15	12	29	30. 1	59. 7	49.49	31					E S E to S E	Saw a large Iſle of Ice. Pleaſant Weather.
		9	27	30									
		6	26	30. 1									
	16	12	27	30. 1	60.14	55. 3	33					S E by S	Freſh Gales and cloudy, with ſome Rain.
		9	27	29. 9									
		6	30	31									
	17	12	26	29. 9	61. 7	59 43	36					S by E to S E by S	Moderate, but hazy, with Rain.
		9	26	29. 9									
		6	31	29. 9									
	18	12	28	29. 5	61.23	63. 7	39					S E	Hazy, with Rain and Fogs. Several Pieces of Ice.
		9	27	29. 7									
		6	27	29.85									
	19	12	31	30	61.47	63.16	39	61.44	61.49	61.54		S E S S W S by E	Foggy, with Calms. Much Ice all round *Reſolution*.
		9	30	29 9									
		6	28	30. 1									
	20	12	29	30	61 40	64. 8	39			61.46		W N W Wt SWbS	Moderate Weather, moſt part Foggy.
		9	30	29. 8									
		6	29	29. 8									
	21	12	29	30. 6	61	64 34	39	61. 4	61. 5	61.12		Wt W N W N by W	Much Ice all round; fore Part Foggy, latter clear.
		9	29	30. 8									
		6	32	29									
	22	12	27	29. 4	61.11	65.40	40		61.	61.12	61.12	N by W	The moſt part foggy and hazy Weather.
		9	28	29 9									
		6	30	29. 9									
	23	12	27	29. 8	61.30	66.50	40			61.30	61.30	So S W	Fair, with much ſhattered Ice, and large Iſles.
		9											
		6											
	24	12	24	29. 8	61.37	67.19	41	61.36		61 38		W N W	Eaſy Breezes, fair and clear, heavy Ice all round.
		9	26	29. 6									
		6	28. 5	29. 9									
	25	12	27	29. 6	61.53	68.12	42					Ditto	Hazy middle Part, clear the latter.
		9	28	29. 8									
		6	28	29. 7									
	26	12	27	29. 8	61.41	68.42	41					SWbW	Fair pleaſant Weather; much Ice.
		9	30	39. 9									
		6											
	27	12	29	29. 7	61.30	68.42	41					N N E W S W	Little Winds, and fair pleaſant Weather.
		9	29	29. 6									
		6	31	29. 8									
	28	12	29	29. 6	61.33	69. 2	40						Moſt Part Foggy, with Calms.
		9	29	29.75									
		6	28	29. 5									
	29	12	27	29. 6	61.30	69.13	40	61.30		61.30	61.20	E S E	Several large Iſles of Ice; Foggy.
		9	25	29 75									
		6	26	29. 9									
	30	12	26. 5	29. 5	61.33	69.26	40					E S E to W S W	Little Winds and Calm; heavy Ice all round.
		9	27	19 9									
		6	29	29. 9									
July	1	12	27. 5	29. 7	61.28	70.10	41					E S E	Faſt in thick Ice, with Fogs and Rain.
		9	27. 5	29. 6									
		6	32	29. 1									
	2	12	31	29	61.28	70.37	41					S E to N E	Theſe 24 Hours the firſt and latter Parts freſh Gales, middle a Storm of Wind and Rain.
		9	26. 5	29. 3									
		6	29. 5	29. 5									
	3	12	28	29. 8	61.28	70.22	41					N W	Hard Gales, with much Snow.
		9	27. 5	29. 9									
		6	32	29 9									
	4	12	29. 5	29. 6	61.28	70.26	40					N W N by W	Moderate and fair Weather with much Ice.
		9	25. 5	29. 3									
		6	29	29. 8									
	5	12	27	29. 9	61.39	70.36	40	61.33		61.44	61.43	W by S	Working to Windward in Ice, ſometimes in a Clear.
		9	29	29. 7									

Month.	Day.	Hour.	Altitude of the Thermometer. ° ′	Altitude of the Barometer. ° ′	Latitude by Account. ° ′	Longitude from *London* by Account. ° ′	Variat. of the Comp. W.	Latitude obſerved by *Smith's* Quadrant. ° ′	Latitude obſerved by *Hadley's* Quadrant. ° ′	Latitude obſerved by *Elton's* Quadrant. ° ′	Latitude by a Sextant of *Ward* and *Smith*. ° ′	Winds.	Remarks.
		6	31	29. 9									
July	6	12	27. 5	29. 7	61.46	70 54	40		61.54		61.53	W S W to NEbN	Moſt Part fair and pleaſant Weather, with much Ice.
		9	26	29. 7									
		6	27	29. 4									
	7	12	27	29. 6	61.55	71.13	40					N E to NWbN	Foggy Weather, cloſe Ice all round.
		9	27	29. 9									
		6	29	29. 7									
	8	12	26	29,75	61.46	70.21	40					Ditto.	Moderate, with Fogs.
		9	25. 5	29. 9									
		6	29	29. 9									
	9	12	27	30	61.42	71.40	41			61.38		Ditto.	The firſt Part hazy, the latter clear.
		9	27.	30									
		6	32. 5	29. 7									
	10	12	29. 5	29. 6	62.18	75.13	41					N W to S E	Clear Sea, freſh Gales and hazy.
		9	26	29.75									
		6	31	29. 7									
	11	12	27	29. 9	62.31	76	42					S by W	Foggy Weather.
		9	28	29.75									
		6	31. 5	29. 9									
	12	12	31. 5	29. 9	63. 5	75.28						Ditto.	Ditto.
		9	28	30									
		6	30	30. 2									
	13	12	29	30. 3	63.15	75				63.20		S to N W	Working in looſe Ice; hazy, ſometimes clear.
		9	27. 5	29. 9									
		6	31	30									
	14	12	27	30	63.25	74.38						N W to Weſt	Much Ice, fair and clear, with Calms.
		9	25	30									
		6	30	29. 9									
	15	12	27	29. 8	63.20	76						S S E to E S E	Much Ice, freſh Gales and hazy.
		9	27	29. 8									
		6	27	29. 8									
	16	12	26	29. 7	63.10	77.20						S E	Looſe Ice, large Clears in Sight of *Saliſbury*.
		9	26 5	29. 7									
		6	27. 5	29. 7									
	17	12	27. 5	29. 6	63.10	78.20	44					E S E	Foggy, much Ice and freſh Gales.
		9	26	29. 7									
		6	29	29, 6									
	18	12	26	29 8	63	78	43					Calm.	Looſe Ice, Eaſt End of *Nottingham*, N b E ſeven Leagues.
		9	26	29. 8									
		6	25	29. 9									
	19	12	24	29.85	63.10	78.20	43					E S E	Much Ice, clear Weather. Cape *Walſingham* S W b W five or ſix Leagues.
		9	27	29.80									
		6	28	29.85									
	20	12	27. 5	29.84	63.20	78 30	41					S W	Shattered Ice, foggy Weſt; Iſland *Diggs*, W S W five Leagues.
		9	27	29. 9									
		6	27. 5	29.98									
	21	12	25. 5	29. 9	63.10	79.50	42					S S E S by W	Cape *Diggs* S E b S 6 Leagues. Freſh Gales, and Ice.
		9	24	29. 8									
		6	27	29 9									
	22	12	25	29. 9	63	80.13	42					SWbW to South.	Much large Ice; the North End of *Mansfield* S W b W four Miles.
		9	23. 5	30. 1									
		6	26. 5	30. 1									
	23	12	26	30	63. 4	81.10	44			62.52		S W	North End of *Mansfield* S W fourteen Miles.
		9	25	30									
		6	29. 5	29. 9									
	24	12	29. 5	29. 9	61.34	82.52	40			61.26		E by S to S S E	Hazy; ſome Pieces of Ice.
		9	26	29.85									
		6	31	29.82									
	25	12	28	29.82	61.15	83.56	38			61.14	61.17	S S W S by E	Much Ice, and Rain.
		9	26	29.83									
		6	32	29 7									
	26	12	29	29. 7	60.45	84.23				60.41		S by E to W by N	Thick foggy Weather, with Showers of Rain.
		9	29. 5	29. 9									
		6	30. 5	29. 9									
	27	12	26	29. 9	59 39	84.11	30	59.36		59.34		N W to Weſt.	Steering Ice with wet Fog.
		9	26	30. 1									
		6	28	30. 1									
	28	12	26	30. 1	58. 9	82.44	28	58. 5	58. 2			N by W	*Baker's Dozens* S b W four Leagues. Fair Weather.
		9	26	30. 1									
		6	28	30									
	29	12	26	30	56.46	82.45	26			56.30		W by S	Moderate, with Fogs, Rain and Ice.
		9	25	29.95									
		6	27. 5	29.85									
	30	12	25. 5	29. 9	55.51	82.39	25					N W b W	Freſh Gales. A great Sea from the Southward.
		9	23	30									
		6	26	29. 9									
	31	12	25. 5	29. 5	54.44	82.47	24					N E	Thunder and Rain. *North-Bear* S W by W five Miles.
		9	24	30									
		6	26	30									
Auguſt	1	12	25. 5	29 98	53. 3	81.20	22					N E	Moderate and fair Weather.
		9	23	29.85									
		6	24	29. 8									
	2	12	23. 5	29. 9	52.20	82						South to North.	The firſt Part moderate, the latter hard Gales, with Thunder and Lightning in with the W. Main.
		9	24	29. 9									
		6											
	3	12	26	30	51.40	83							Arrived in *Mooſe-River* Road.
		9											
		6											
	4	12											
		9											
Sept.	1	12	27	29.85	55 36	Weſt. 00.42	22					Weſt and W by N	Moderate and fair.
		9	29	29.85									
	2	12	27	30. 5	53.50	from *Bear.* 1.16	26					N W to North.	Moderate Gales and Hazy, with ſmall Rain.
		9	26	30. 8									
		9	30	29. 8									
	3	12	26	30. 4	56. 6	1.16	26					N by W to N by E	Ditto.
		9	23	31. 5									
		9	30	29. 8									
	4	12	26	30. 6	58. 6	2.58	30					South to Eaſt.	The firſt Part moderate, middle and latter very hard Gales, with Squalls of Rain.
		9	23	31									
		9	33	29. 8									
	5	12	33	29.85	58.28	4.26	30					N E b E	Ditto, hard Gales and Squalls.
		9	34	30									
		9	33	30. 1									
	6	12	32. 5	30. 2	58.40	3.23	31	58.42	58.49			E N E North.	Freſh Gales and ſqually, with a Head-Sea.
		9	32	30. 2									

Month.	Day.	Hour.	Altitude of the Thermometer.	Altitude of the Barometer.	Latitude by Account.	Longitude from London by Account.	Variat. of the Comp.	Latitude observed by Smith's Quadrant.	Latitude observed by Hadley's Quadrant.	Latitude observed by Elton's Quadrant.	Latitude by a Sextant of Ward and Smith.	Winds.	Remarks.
			° ′	° ′	° ′	° ′	W.	° ′	° ′	° ′	° ′		
		9	27	29. 9									
Sept.	7	12	27	29. 8	60.45	1.15	31					N W	Ditto, Gales and Snow.
		9	26	30. 1									
		9	37. 5	30. 3									
	8	12	35	30. 2	61.37	East. 0. 4	32					S W to N W	Strong Gales and squally; great Sea from the North-West.
		9	32	30. 1									
		9	34	30. 3									
	9	12	32. 5	30. 3	61.55	East. 0. 4	33					N W to N N E	Moderate, and fair; four Leagues from the South End of *Mansfields* 76 Fathom, Mud.
		9	32	30. 5									
		9	32	30. 5									
	10	12	29	30. 5	61.58	West. 1. 0	34	61.43	61.45	61 46		N by E S W	Little Winds and fair Weather. In Sight of *Mansfields*.
		9	30	30. 5									
		9	36	30.25									
	11	12	30	30. 1	62.30	East. 2.21	43	61.30				S W	Fair and pleasant Weather. In Sight of *Mansfields*, 4. Miles.
		9	28	30									
		9	30	30. 1									
	12	12	30	30	62.52	East. 2.28	42					W S W to W N W	At Noon *Mansfields* N W by W 5 Leagues; Sleet and Foggy. Cape *Walsingham* S W 6 Leagues.
		9	30	30. 1									
		9	32	30. 3									
	13	12	33	30.25	63. 9	fr. Digs 3.40 East.	43					N W to N by E	Frequent Showers of Hail and Snow.
		9	32	30. 2									
		9	31. 5	30. 1									
	14	12	29	30. 5	62.50	East. 6.44	42					N N E Calm.	Moderate and fair Weather.
		9	31	30									
		9	30. 5	29. 5									
	15	12	30	29.46	62.22	7.55	42	62.21	62.23			S W to W N W	Much Snow, with thick Weather.
		9	29	29. 5									
		9	29	29. 8									
	16	12	30	30	61.43	12	42					N W to N by E	Passed several large Isles of Ice; fresh Gales and Sleet; thick Weather with Snow.
		9	28	30. 3									
		9	32. 5	30. 4									
	17	12	33	30. 5	61.43	15.10	40					North. to N N E	First 2 Parts hard Gales and Snow, latter moderate. At Noon Cape *Resolut.* NE6L. S. Pt. NWbN4L.
		9	31	30.55									
		9	31	30. 4									
	18	12	34	30. 3	60.40	West. 58.40	40					NEbN	Many large Isles of Ice; hard Gales; great Sea from the North-East.
		9	31	30. 4									
		9	23	29. 9									
	19	12	26	29. 5	59.49	53.43	38					North. to S W	Moderate, but dark and cloudy, with Sleet and Rain.
		9	26	29. 5									
		9	30	29. 7									
	20	12	29	29.65	58.42	48.52	32	58.37	58.38			N W	Fresh Gales, with Rain and Snow.
		9	29	29. 6									
		9	31	29. 5									
	21	12	30	29. 6	57.28	44.31	29					N N W to North.	Variable, with fresh Gales, and Rain.
		9	29	27. 7									
		9	27. 5	29. 5									
	22	12	27. 5	29. 5	56.49	41.39	27					North. to E S E	Fresh Gales and Squalls; variable Winds and cloudy.
		9	27	29. 6									
		9	29	29.65									
Sept.	23	12	26. 5	29. 5	55.52	36.50	25	55.44	55.47			N E b E to N by W	Hard Gales, with Rain and Fogs.
		9	26	29. 7									
		9	31	29. 8									
	24	12	27	39. 9	55. 3	31.21	24	55. 1	55. 4			N N W	Fresh Gales, and Rain.
		9	26	29. 8									
		9	27. 5	30									
	25	12	26	30	53.43	25.59	23					N W b N to S W b W	Hard Gales, with Squalls and small Rain.
		9	26	30. 2									
		9	21	30. 2									
	26	12	19	30. 2	52.38	22.23	22					S W to S W b S	First Part hard Gales, middle and latter moderate and hazy.
		9	20	30. 1									
		9	21	30									
	27	12	20. 5	31	52.27	20.15	21					South to N N E	Fresh Gales, and much Rain; hazy Weather.
		9	24	30. 1									
		9	25	30. 2									
	28	12	25	30.25	50.30	16.33	20	51	51			N E	Fresh Gales, and much Rain; hazy Weather.
		9	24	30. 2									
		9	23	30. 3									
	29	12	23	30. 2	50.20	13. 7	19	50.24	50.25			N E to N by W	Fresh Gales and hazy, with some Rain the first two Parts, the latter moderate.
		9	24	30. 2									
		9	23	30. 2									
	30	12	21. 5	30. 3	50.37	11.41	18	50.30	50.35			N W to S by E	Moderate and fair; Winds variable.
		9	20	30. 2									
		9	22. 5	30. 3									
Octob.	1	12	22	30. 2	50.35	11.22	18					S S E	Moderate, with a great Head Sea from the Eastward.
		9	22	30. 3									
		9	22	30. 3									
	2	12	23	30. 2	49.40	12.32	18					S E by S	Cloudy, with a great Sea from the Eastward.
		9	22	30									
		9	22	29									
	3	12	22. 5	29	50. 7	11.39	18					S S E to S by E	Hard Storm of Wind, with great Sea.
		9	22. 5	30. 1									
		9	20	29. 9									
	4	12	20	30. 1	50. 3	10.36	17	50. 4				S S W	Moderate Gales, with small Rain.
		9	20	30.25									
		9	20. 5	30.35									
	5	12	20. 5	30. 3	49 38	8,52	16	49 42				S S W to West	Ditto, fair and moderate.
		9	20. 5	30. 4									
		9	22	30. 2									
	6	12	22	30. 1	50	7.20	15	50. 5				South Calm S S E	Saw several Ships, sounded 100 Fathom. Moderate.
		9	22	29. 9									
		9	22	29. 9									
	7	12	21	30	49.39	6.58	15	49.38				S S E WNW	Two first Parts hard Gales, heavy Squalls, the latter moderate.
		9	21	29. 9									

An Explanation of the TABLES.

The first Column contains the Month; the second Column is the Day of the Month; the third the Hour of the Day, beginning at 6 in the Morning, to 12 at Noon, and 9 at Night; the fourth Column is that of the Thermoscope; the fifth Column is the Height of the Mercury in the Baroscope, the first Number is the Inches of it's Height, the second and third Number marks the tenths and hundredth Parts of an Inch; the sixth Column is the Latitude the Ship is in, by Account, every Day at Noon; the seventh Column is the Longitude the Ship is in every Day at Noon, by Account, from the Meridian of *London* (except where otherwise expressed). The Column Variation, is the Variation of the Needle; aud the next four Columns are the Latitudes observed at Noon by four several new Instruments; the first is Mr *Smith*'s Prismatic Quadrant, the second is Mr *Hadley*'s, the third by Mr *John Elton*, and the fourth by Mr *Caleb Smith* and Mr *William Ward*; the next Column is the Wind for the most Part of the 24 Hours.

The Thermoscope which I made use of in the Voyage, was made by Mr *John Patrick*, together with the Baroscope; in his Thermoscope he places [°] at the Top, supposing it to be the Heat under the Line, and so the Figures increase downwards, with the Increase of Cold. Temperate is placed at 25.

This Prismatic Quadrant of Mr *Caleb Smith* I find to be of very great Use at Sea, in particular for the Stars, as I have experienced several times in my Voyage to *Hudson*'s *Bay*, in the worst of Weather, when you can but see the Horizon; and his other is of great Use, in tolerable smooth Water, in foggy and hazy Weather, when there is no Horizon to be seen, yet have the Benefit of the Sun.